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PERSONALITY AND LIFE HISTORY FACTORS IN PERSONS WHO ARE POTENTIALLY HYPERTENSIVE[1]

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Clinical psychiatric studies of persons with essential hypertension have been offered to support the theory that psychological factors influence the development and course of hypertensive disease. If these factors are more than incidental or secondary, then they must exist before the onset of disease; *i.e.*, it should be possible to demonstrate that young adults who are predisposed to develop hypertension differ in psychological functioning from persons who do not become hypertensive. Repeated observations of persons so disposed may be utilized to study the developmental aspects of psychophysiological relations and to trace the sequence of events leading to the onset of hypertension.

The predisposition to essential hypertension can be identified in young adults because an elevated blood pressure, even though transient, has prognostic significance. For example, Hines (11), Diehl and Hesdorffer (5), and Levy *et al.* (13) have demonstrated an increasing incidence of hypertension over time as a function of systolic and diastolic pressures recorded in early adult life. The relationship is high enough to warrant designating a group of young adults with at least transiently elevated blood pressure "prehypertensive," even though not all of them will become hypertensive in later life. The term simply implies a higher-than-average actuarial probability of later hypertension among a group of persons; it does not necessarily imply that any particular person is in an early stage of the disease, or that he will inevitably develop hypertension.

Aside from the logical necessity of utilizing subjects with the predisposition to essential hypertension rather than hypertensive patients to trace the pathogenesis of the disease, there is an important methodological advantage. Studies may be conducted on a double-blind basis, *i.e.*, the subjects need not be informed of the criteria for their selection and the research staff need not know, until all of their judgments have been recorded, which subjects are prehypertensive and which are normal. Such a control is necessary when the data are obtained from interviews; otherwise, theoretical preconceptions may seriously bias the collection and interpretation of information.

Four years before the study reported here, Harris, Sokolow, Carpenter, Freedman and Hunt (10) selected a group of college women whose blood pressures exceeded 140 mm. Hg systolic and/or 90 diastolic during their matriculation physical examinations. These "prehypertensives" were matched with controls whose pressures were less than 120/80. Both groups were exposed to emotion-provoking, interpersonally stressful sit-

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uations (psychodramas). The prehypertensives were found to be less well controlled, more impulsive, more egocentric, and generally less adaptable than the controls. The results suggested that they would be vulnerable in real-life situations involving psychological stress, and hence frequently subject to the autonomic components of emotion, including repetitive rises of blood pressure, which may initiate the processes leading to hypertension.

Because the prehypertensives could be expected to have difficulty in meeting the problems associated with leaving college and establishing themselves in adult roles, and because confirmation of the stability of personality characteristics over time would help to validate their relationship to the blood pressure classifications, a follow-up of the same young women was undertaken.

SUBJECTS AND METHOD

Life history interviews were conducted with 20 prehypertensive subjects and 22 controls, approximately half of the subjects from each group in the original study. There appeared to be no differentially selective factors governing their return for the follow-up. The average age of both groups was 24 and one-half years (range 22-1 to 33-7) at the time of the follow-up. The subjects were informed that our interest centered in relationships between emotions and physical health, but they were not told and probably were not aware of our specific interest in blood pressure.

THE INTERVIEW SITUATION

The interviews were conducted in a three-person setting involving two female staff members, one a psychiatrist guiding the interaction and the other a clinical psychologist acting primarily as observer and participating minimally. The interviews usually began with a discussion of the subject's current life situation, and the order of subsequent topics was left largely to the subject herself, with the interviewer posing a minimal number of questions. Since the interviewer did not know which subjects were prehypertensive and which control, she had to be equally vigilant with all subjects in exploring an area presumed relevant to the psychodynamics of hypertension.

Questions regarding the effect that the presence of an observer might have on rapport, on the kind and quantity of material elicited, and on constraints for interviewer and interviewee alike were balanced against concern regarding the reliability of interpersonal observations by a single interviewer. It is obvious that two- and three-person interview situations are different, and the study was not designed to explore the nature of such differences. It was decided to include an observer on the assumption that her presence would not affect the two groups differentially, and that the increased validity provided by two sets of observations would offset any possible loss of information.

In practice, the three-person situation did not present any unusual problems. The interviewer and observer had worked together over a long period of time and consequently were more comfortable about the situation and the observations they were to make than would otherwise have been the case. As far as the subjects were concerned, with very few exceptions they ceased to attend to or seem aware of the observer, once the interview was under way. For the exceptions, all of whom turned out to be prehypertensive subjects, continuing attention to the observer was a revealing aspect of their interpersonal interactions. A quotation from the interviewer's narrative summary illustrates the interaction with one of these subjects whose behavior typified that of a number of prehypertensives:

"She has a competent, matter-of-fact manner and a disconcerting way of responding to questions as if they were none of the examiner's business. She prefaces her replies with a kind of sneering, surprised pause as if the examiner had said something

incredibly rude and then addresses herself to the observer, implying that only the latter is worth talking to and will bear witness to her taking this laughable situation in her stride. In view of the voluntary nature of the interview, her attitude is grossly inappropriate. When she persists in this response for some minutes she is asked if she wishes to go on with the interview, since she seems so reluctant to give information about herself. She meets this question with the same surprise and disgust but subsequently becomes somewhat less hostile, although she maintains the friend-enemy dichotomy between observer and examiner."

RECORDING THE INTERVIEW OBSERVATIONS

The interviewer and observer independently recorded their impressions immediately after each interview, using the following methods:

Rating Scales: Sixty-six six-point rating scales were specially developed to reflect as many aspects of personality (both overt and inferred) as practicable, with particular attention to variables presumably relevant to the psychodynamics of hypertension. Differences between the groups were tested for significance by Fisher's *t*.

Adjective Check List: A list of 274 common adjectives permitted description of the subjects in relatively non-technical, non-inferential terms. Differences between the groups in the frequency of checks for each adjective were tested for significance by the standard error of a difference between proportions.

Q-Sort Descriptions: One hundred and three statements descriptive of personality and behavior were modified for the present study from a set developed by Block (2). These statements, or Q-sort items, were sorted from most to least characteristic for each subject in a fixed, approximately normal nine-point distribution. The sortings for the two observers were combined by summing the values for each item, first for individuals and then for the groups. The totals were redistributed in the same nine-step arrangement to form composites for each group. These composite Q-sorts permitted two kinds of inspection. First, the composite for each group was reviewed to determine the kinds of items falling at the extremes, the most and least characteristic items. Second, the composites were examined for items which were placed significantly higher or lower for one or the other group, either at the extremes or in the middle of the range. The stability of the items within a composite was estimated from the standard error of measurement. A difference of two steps (on the nine-point distribution) between composites is significant at approximately the .05 level as estimated from the stability of placements and the variability within groups. The median reliability of the Q-sorts for individuals, based on two observers, was .77. The reliability of the group composites was estimated at .80.

Overall Prediction: The interviewer and observer made a judgment (on a scale from 0–3) as to whether each subject was prehypertensive or control. Their predictions were based on whatever hypotheses they held about the relation of psychological factors to hypertension, but the findings of the present study, of course, were not available to them. The two ratings were averaged and the difference between the means of the groups tested for significance with Fisher's *t*.

Clinical Descriptions: The interviewer and observer also wrote narrative summaries, recording idiosyncratic material which could not always be coded by the more objective methods. Quotations from these summaries are used for illustrative purposes.

The fact that many significance tests were performed, several hundred in all, limits their value for assessing the probability of a "true" difference on any one variable. We were, however, not seeking to test a particular hypothesis with each of

the numerous rating scales, adjectives and Q-sort items. The interpretations which are made depend upon several variables considered together, and no single variable is critical for an interpretation. For these reasons, significance levels are not listed in the tables presented below. Two-thirds of the differences reported are significant beyond the .05 level, a third is between the .05 and .10 levels. Where the word "significant" appears, a test of statistical significance is implied.

RESULTS

Analysis of the interview data demonstrated, as had the previous investigation, that the prehypertensives differed in their personality and behavior from the controls. The interviewers' overall predictions of whether each subject was prehypertensive or control significantly separated the groups ($t = 2.02, p < .05$). A different interviewer in the first study achieved a separation at a similar level of significance ($X^2 = 5.1$, $p < .02$) with a somewhat different sampling of cases from the original groups.

ADJECTIVE CHECK LIST

Table 1 lists the adjectives which the interviewers checked significantly more often for each of the groups. The descriptions of the prehypertensives are almost identical with those from the initial study, and the original categorization is equally appropriate for the present list:

1. The majority of the adjectives have a negative tone and describe personality in unfavorable terms; for example, irritable, unsympathetic, self-centered, resentful and awkward.

2. Many of the adjectives refer to inadequate control of behavior or emotionality; for example, changeable, erratic, excitable and restless.

3. The words describe an egocentric kind of person; for example, boastful, self-centered, unsympathetic.

4. Other words suggest moodiness; for example, resentful, sour, and sulky. The terms "affectionate" and "hospitable," in the context of this list, may imply the kind of ineffective search for close interpersonal relationships that Erikson (6) has termed "affiliative hunger." It was this kind of immature, awkward reaching out that was observed in the prehypertensives.

The adjectives describing the control subjects are strikingly different and can also be categorized as in the initial study:

1. The words are positively toned, "nice" things to say about people; for example, gentle, helpful, modest, pleasant, poised.

TABLE 1

Adjectives That Differentiate the Prehypertensive and Control Groups

Checked Significantly More Often for the Prehypertensives			
Affectionate	Defensive	Hospitable	Resentful
Aggressive	Dominant	Impatient	Restless
Awkward	Erratic	Irritable	Self-centered
Boastful	Excitable	Nervous	Sour
Changeable	Foolish	Quick	Sulky
Cynical	Hasty	Rash	Unsympathetic
Checked Significantly More Often for the Controls			
Calm	Docile	Meek	Practical
Cautious	Efficient	Moderate	Quiet
Civilized	Feminine	Modest	Reasonable
Conforming	Gentle	Painstaking	Reserved
Conservative	Helpful	Patient	Restrained
Conventional	Leisurely	Pleasant	Self-assured
Discreet	Mannerly	Poised	Sophisticated

TABLE 2

Interview Ratings That Significantly Differentiate the Prehypertensive and the Control Subjects

More Characteristic of Prehypertensives	*More Characteristic of Controls*
Loose parental discipline	Stability of present adjustment
Differentiated perception of family relationships	Mature femininity
Residuals of early psychosexual stages: oral aggressive	Inferred self-confidence
Verbosity of speech	Reaction pattern to life stress: flight
Pervasiveness of superego	Reaction pattern to life stress: repression
Discrepancy between inferred and reported self-confidence (reported self-confidence greater than inferred by observers)	Neatness of dress
	Attention to dress
	Femininity of posture
	Straightness of posture
	Residuals of early psychosexual stages: anal retentive
	Tolerance of superego

2. Words denoting impulsiveness are absent, and in their place are words suggesting tempered control; for example, efficient, conservative, moderate, restrained.

3. More words than in the prehypertensive list describe relations with other people; for example, gentle, helpful, pleasant, reasonable.

4. The words describe persons with social poise; for example, sophisticated, calm, civilized, self-assured.

The correspondence of these results with the earlier ones indicates the stability of the group characteristics over time and also with different observers and procedures, psychodramas in the first study and interviews in the present one.

RATING SCALES

Table 2 lists the variables on which each group was rated significantly higher than the other. It is apparent that some of the scales refer to such external attributes as neatness of dress, and others to inferences from behavior and life history data, for example, residuals of the psychosexual stages of development.

The prehypertensives remembered their parents' discipline as loose (overly permissive), and they developed, perhaps reactively, pervasive superegos. Although they were rated higher on "differentiated perception of family relationships," this variable probably reflects repressiveness in the controls rather than unusually keen understanding in the prehypertensives. Their high ratings on oral aggressiveness are further evidenced by the fact that several of them had ragged, bitten fingernails, and by adjective descriptions such as aggressive, cynical, sour, sulky, etc. Finally, the discrepancy between self-confidence as claimed by the subjects and as inferred by the observers, suggests a pretentious facade which could not be sustained through the interview.

In contrast, the picture which emerged for the controls was one of stability, poise and femininity, but at the cost of some constriction of personality (retentive residuals) and refusal (through flight and repression) to deal with certain potentially disturbing material. They were seen as self-confident, and their tolerant superegos contrast with the prehypertensives' readiness to feel guilty.

Q-SORT DESCRIPTIONS

The composite Q-sorts provide information on the ordering or relative importance of the 103 items for each of the two groups. Tables 3 and 4 list the items falling at the extremes of the composites, the most and least characteristic items. The latter can be most easily comprehended by prefacing each one with a negation, for example,

"*Does not* . . . give up easily," and "*Is not* . . . readily dominated by others." Table 5 lists the items whose average placement was two or more steps higher (significant at about the .05 level) for one group than the other. The findings in these tables can be summarized in three interrelated clusters: attitudes toward self and culturally prescribed role, assertive and hostile impulses, and adequacy of defenses.

TABLE 3

*Most Characteristic Q-Sort Items in the Prehypertensive and Control Composites**

Prehypertensive	*Control*
Shows conflict about sexual identification	Is cooperative; eager to please
Is nervous, tense in manner; trembles, flushes, sweats or shows other evidence of anxiety and embarrassment	Prides self on being "objective," rational
Behaves in an assertive fashion	Is emotionally constricted
Has hostility toward others	Is feminine in her style and manner of behavior
Has anxiety	Is physically attractive; goodlooking

* Table does not list items that fell at the extreme for both groups.

TABLE 4

*Least Characteristic Q-Sort Items in the Prehypertensive and Control Composites**

Prehypertensive	*Control*
Gives up easily	Behaves in an ostentatious and exhibitionistic manner
Is readily dominated by others; submissive	Extrapunitive; tends to transfer blame
Is relaxed, easy-going	Is erratic, unpredictable
Is bland, emotionless in approach to situations	Is sarcastic
Has passive attitude toward environment	Her various needs tend toward relatively direct and uncontrolled expression; unable to delay gratification; acts out
Satisfied with herself; is not subjectively aware of self-concern	Is self-dramatizing; histrionic
Accepts others as they are; not judgmental	Self-pitying; feels cheated and victimized by life

* Table does not list items that fell at the extreme for both groups.

TABLE 5

*Other Q-Sort Items Two or More Steps Different in Prehypertensive and Control Composites**

More Characteristic of Prehypertensives	*More Characteristic of Controls*
Is self-abasing; tends to assume blame	Is overly dependent; seeks help; refrains from doing things herself
Tends to be self-defensive; anticipates being attacked and criticized	Feels separate, unconnected, detached; feels a lack of personal meaning in her life
Has a rapid personal tempo; behaves and acts quickly	Tends to be conciliatory in her interpersonal relationships; accepts and fosters compromises; tends to be appeasing
Has a brittle ego-defense system; small margin of integration; precarious self-control	Is moralistic and strict
Is sensitive to anything that can be construed as a demand	Judges self and others in conventional terms like "popularity," the "correct thing to do," social pressures, etc.
Is restless, distractible	Uses obsessive-compulsive defenses
	Handles anxiety and conflicts by refusing to recognize their presence (repression)
	Has social poise and presence; appears socially at ease

* Items included in Tables 3 and 4 are not listed. A two-step difference is significant at about the .05 level.

Attitudes Toward Self and Culturally Prescribed Role: The prehypertensives showed significantly more conflict about sexual identification. They appeared to be dissatisfied with themselves and others, and overly sensitive to real or imagined criticism. The controls, on the other hand, were seen as conventionally feminine. Apparently they have accepted contemporary social norms appropriate to their age and sex, and evaluate themselves and others according to conventional standards of propriety and moral conduct.

It was the interviewer's impression that the achievement of identity was a continuing problem for the prehypertensives. The ages of the subjects determined that identity conflicts were most clearly seen in failure to accept a sex-appropriate role, since this aspect of identity is central to young adult life (7). Failure to accept parental models and a tendency to identify with the unlike-sex parent express these conflicts in the interviewer's summaries of two prehypertensive subjects:

"Apparently Miss A has identified rather well with her father and is following in his footsteps—his values, his profession. Her identification with her mother has not fared so well. Her appearance reveals something of her attitudes about being a woman. Although she obviously spent some time getting herself together today, the result is one of studied poor taste. One can't ascribe it to indifference to style alone; it's as if she were unconsciously proclaiming her opinion that women are foolish. It is interesting to note that she says her mother criticizes her for not giving her dress more attention. Perhaps, unconsciously, she would rather dress like a man. In this case also, selecting her father's occupation could represent a contemptuous repudiation of her mother. Although she gives a token acceptance to getting married sometime in the future, she may put it off indefinitely by achieving impasses such as she has reached with her current boy-friend."

"Miss L. has been busily brushing off any noticeable similarity to her parents ever since she caught on to their ambition for her to do just that. She does it with vengeance and an unknown quantity of guilt feelings. She seems to escape self-recrimination by letting her parents do it for her. She tries so hard not to be like them that she really doesn't know who she is, but she escapes this discomfort by labeling herself an intellectual and an esthete. Although she allows herself almost no introspection, she is partially aware of her interpersonal isolation. Her apparently inflexible social behavior of amused, patronizing superiority discourages interaction, unless one 'fights' with her as her mother does. Offense being a very good defense, she is well protected but also cut off from interpersonal learning experiences and the development of social skills."

Assertiveness and Hostility: The prehypertensives were seen as assertive, relatively independent, unwilling to accept the dominance of others, and struggling actively with their environments. In contrast, the controls appeared cooperative and conciliatory in their personal relationships. The prehypertensives' assertiveness was fused with hostility and was directed against other people, whom they tended to see as critical and demanding, but it was usually not expressed appropriately in context. Hostility was also directed inwardly in self-dissatisfaction and self-abasive attitudes. These points are illustrated in a note on a prehypertensive subject:

"Although she indicates that she is good-naturedly ready for anything, at the same time she paradoxically radiates exasperation. This exasperation is diffuse and essentially self-directed, although one infers she is fairly frequently annoyed with her environment, too. Her resentments are chiefly toward herself. Although she indicates that she takes her temper for granted and is not dismayed when she lets it fly, which seems to be frequently, one infers that she is un-

easy about self-control of hostility, also. She is relatively unaware of her negative feelings toward her mother, although she seems to think that she has no difficulty in expressing hostility, just in suppressing it. Her hostility has a more self-punitive than 'expressive' quality, in that it serves to put herself in a 'bad light.' She looks upon herself as an immature, undisciplined person, and she does seem to be, but she cannot be happily idle, happily self-indulgent, or really carefree. Her easily mobilized anger seems to spring from a reservoir of hostility related to chronically frustrated dependent needs."

Adequacy of Defenses: The prehypertensives were relatively poorly buffered against both internal impulses and threats they perceived in external reality. Their brittle ego-defense systems and precarious self-control were reflected in overt anxiety, muscular tension, signs of autonomic discharge, and generally high levels of emotional reactivity. In contrast, the controls were seen as "objective" and "rational," able to delay gratification of impulse and not erratic or acting-out. Relatively untroubled by socially unacceptable impulses and less inclined to attribute hostility to their environment, they were able to establish at least superficially conflict-free relations with other people. Neurotic defenses (repression and obsessive-compulsive mechanisms) were more available to them than to the prehypertensives and, perhaps as a consequence, they appeared to be somewhat constricted, dissociated, detached and colorless. One of the narrative summaries illustrates these characteristics:

"This is a neat, organized, assured wife and mother whose manner is easy, friendly, capable and matter-of-fact. She is dressed appropriately for the (warm) weather in a neat, pretty, black and white print and trim red pumps. Her black hair and black-rimmed glasses are attractive, her bare arms and shoulders are nicely tanned, and the whole effect is smooth and pleasing. She has no problems with herself at present except that she does not accomplish enough; she is 'not one to sit and look at TV all evening.' Although she was graduated from college she has no particular wish to take on a career at any future time. She feels that she is a good mother—permissive with her children, with successful results in regard to toilet training and feeding—unlike some 'other mothers who worry about such things and therefore create their own problems.' Obviously she makes very good use of repression, but one has the feeling that through her marriage and motherhood she has been able to mature considerably and sublimate a number of difficulties. She appears to be functioning very well interpersonally and with herself."

It may be noted that the prehypertensives' anxiety was less bound in neurotic symptoms. To some extent it was subjectively experienced but more often it was expressed diffusely in motor outlets, as illustrated in the following excerpt:

"This subject, with her big pink hair bow and coy, petulant interactions reminded me of a five-year-old. Indeed, in many ways to judge from the interview material, she has not grown up emotionally. She is undercontrolling, demanding, and intrusive in her dealings with others. The combined qualities of anxiety and irritability engender a negative response in the interviewer which makes it difficult to evaluate the positive aspects of her personality. She admits an inability to read or study for longer than an hour at a time. She is, further, unable to tolerate her own company and has to be 'doing something' whenever she is alone. She says she works only to keep herself busy. The absence of other observable defenses for her pervasive anxiety suggests a rather unstable current adjustment."

OTHER INTERVIEW DATA

The prehypertensives' difficulties in meeting the problems of early adult life are further evidenced by their marital histories.

Significantly more of the controls (59 vs. 25 per cent) were married and had children (36 vs. five per cent) at the time of the follow-up. Eleven of the thirteen married controls were housewives, eight had children, and only two were continuing to work full-time. One of these was separated from her husband and faced the necessity of supporting her children. The other was helping her husband financially while he was finishing college. Sufficient detail regarding each of the marriages was available to indicate that, with the exception of the couple who were separated, there were many positive elements in the relationships and that the marriages would be lasting ones.

In contrast, only one of the prehypertensives had children. Her husband, who was more than 30 years her senior, had been hospitalized, probably permanently, after a cerebral thrombosis. She was receiving public assistance and leading a marginal existence. Two prehypertensives had married men who were still in school. Another was working in a different community from her husband, seeing him only on weekends, and she described the relationship as extremely strained. Another was married after very short acquaintance to a man who was leaving for military service. Two prehypertensives were about to be married when they were interviewed, one of them for a second time. Both had professional training and were marrying professional men. Although it would be premature to attempt to evaluate the stability of the marriages in either group, it appears that by their middle twenties the controls were better able to accept (or achieve) the conventional roles of wife and mother.

DISCUSSION

The present findings serve to confirm and extend previous reports on the association of personality and blood pressure.[4] Not only do young adult women with a history of transiently elevated blood pressure behave characteristically in laboratory-contrived socially stressful situations, but also their modes of psychological functioning continue over a four-year period and can be discerned under different conditions by a second set of observers. Because different methods were utilized in the two studies, the data are not strictly comparable; however, it is clear that on both occasions the subjects with higher pressures (the prehypertensives) differed from normotensive controls on similar personality dimensions. For example, the descriptive categories developed in the first study continue to fit the later observers' impressions of the two groups and the differences between them. Continuity is further evidenced by the observers' tendencies on both occasions to describe the prehypertensives in negatively valued terms. Striking illustrations of the validity of their impressions are provided by the diverging marital histories of the two groups: the prehypertensives could reasonably be predicted to be relatively poor risks in the marital market, to contract less auspicious marriages or to be less likely to marry at all. A review of their current life situations accords well with this expectation. Apparently the controls have adapted more readily to culturally prescribed roles and achieved a closer approxi-

[4] This report is concerned with the psychological characteristics of prehypertensive and control subjects selected on a single occasion of measurement. Validation of the blood pressure classification will require long term follow-up studies to ascertain the incidence of hypertension in the two groups. In the meantime it may be noted that the prehypertensives have shown consistently and significantly higher systolic and diastolic pressures over a period of five to seven years under several conditions of measurement: at the time of their matriculation physical examinations (the basis for their selection); before and after a stressful psychological procedure one to three years later; and, at the time the present study was done, under resting conditions and in response to breathholding and hyperventilation. Further evidence of the greater likelihood of later hypertension in the prehypertensives is the fact that their close relatives have shown significantly higher death rates and their parents a higher incidence of cardiovascular disease.

mation to current norms of mature femininity.

In this connection, it may be noted that another, younger group of subjects, also selected for relatively high causal pressures, showed similar difficulties in achieving age- and sex-appropriate roles (9). Personality data which had been recorded from early childhood through adolescence showed conflicts about sexual identification and disturbances of body image, along with tendencies to be overly dominant and assertive in social situations.[5] Adolescents with lower pressures, like the controls in the present study, were rated as self-confident, optimistic and adaptable.

Personality characteristics like those of the prehypertensives have also been shown to extend into older ages and a higher range of pressures (12). Women with essential hypertension were exposed to emotion-provoking situations like those used with the prehypertensives, modified somewhat for different age, educational and socio-economic levels. Compared with normotensive controls, the hypertensives showed inappropriate assertiveness and hostility, and the results indicated that difficulties in emotional control and relations with other people present pervasive and recurring problems.

The convergence of these results for groups at different ages and different levels of blood pressure strengthens the hypothesis that psychological factors are relevant to the pathogenesis of hypertension. Apparently the same kinds of psychological factors that differentiate hypertensive from normal persons also differentiate young adults and adolescents with pressures in the upper range of the distribution from persons with lower pressures. An important implication is that these factors antedate the onset of hypertension; hence, they cannot be explained as secondary effects of sustained high pressure.

The personality characteristics of the groups with higher pressures suggest an interpretation which links the findings with possible mechanisms underlying the development of sustained high arterial pressure. Both the hypertensives and prehypertensives seem to suffer from defects of socialization which frequently bring them into conflict with other people. They seem overly ready to perceive other people as hostile and demanding, and their own attitudes and behavior tend to elicit the kinds of responses in others which make their interactions stressful. It seems likely that they experience emotional upsets more frequently than normals, and these upsets may be larger and more prolonged. Vasopressor episodes, reflecting the autonomic components of emotional arousal, could be expected to correspond in frequency, magnitude and duration, so that the total pressure integrated over time would be greater than in normals.

Folkow (8) has described mechanisms by which emotionally induced pressor episodes may have "triggering" or "potentiating" effects on the development of hypertension. In the early phases, frequently recurring vasoconstriction may decrease renal blood flow (with consequent engagement of humoral mechanisms) or influence extrarenal processes sufficiently to produce organic arteriolar changes.[6] Intermittent

[5] These findings describe female subjects. Adolescent males in the same study (9) showed similar problems about identification but somewhat different patterns of dominance and assertiveness. The results were derived from individual summaries prepared by Erik H. Erikson from extensive case history materials and put into quantitative form for statistical testing by a Q-sort method. The high and low blood pressure groups were composed by averaging the pressures on three successive annual physical examinations at ages 15 through 17, and selecting the highest and lowest 20 per cent of the range.

[6] Altschule (1) and many others have questioned the relevance of emotionally induced transient elevations to the pathogenesis of hypertension. They argue that such elevations, common in psychoneurotics, are due to increased cardiac output with unchanged peripheral resistance, a pattern opposite to that presumably characteristic of hy-

rises in pressure constitute an increased average pressure load on the resistance vessels, and may gradually initiate secondary vascular wall hypertrophy. In the later phases of hypertension, emotionally induced vasomotor activity even of ordinary degree, superimposed upon already narrowed arterioles, will cause large increases in pressure, thereby accelerating the development of vascular lesions.

Although the details of these processes and even the nature of the final lesion are unknown, it may be noted that Pickering (14) and Smirk (15) have shown that any of a variety of factors can raise the pressure, and that the pressure may remain higher even after the precipitating factor is removed; *i.e.*, the processes underlying the development of high pressure may not be the same as those which sustain it at hypertensive levels. The tendency to respond to many life situations with emotional arousal, including repetitive blood pressure rises, may be another such precipitating factor.

SUMMARY

Young adult women identified as having a higher-than-average probability of becoming hypertensive in later life (prehypertensives) and normal controls, first studied when they were in college, returned four years later for an interview which inquired into personal history, current life situation, and characteristic modes of psychological functioning. Objective methods of recording the interview permitted quantitative testing on variables which previous research had suggested were related to essential hypertension or to the predisposition to develop high blood pressure.

The prehypertensives in their middle twenties were found to be experiencing unusual difficulty in achieving sex- and age-appropriate roles, and their attitudes and behavior seemed likely to provoke frequent stressful interactions with other people. The results confirm those obtained earlier in the same groups of subjects studied with different methods, and also in adolescents with relatively elevated blood pressure (within the normal range) who had been followed from early childhood. Furthermore, the personality characteristics of the prehypertensives were like those observed in older women with essential hypertension, adding further evidence that psychological factors are related to the processes underlying the development of sustained high blood pressure and are not secondary effects of the later phases of hypertension.

The nature of the prehypertensives' (and hypertensives') psychological functioning suggests a low threshold for perceiving and responding to many everyday life situations as if they were the occasion for emotional response. The findings are consistent with the hypothesis that the frequency, magnitude and duration of vasopressor episodes and other autonomic components of emotional response are related to the development of sustained high blood pressure.

pertensives. The argument overlooks the fact that the high resistance, normal output pattern is observed in hypertensives under resting conditions, rather that in response to stimulation. Brod *et al.* (3) found both hemodynamic patterns and mixtures thereof almost equally often in hypertensives and normal controls in response to the cold pressor test and mental arithmetic. Further, Brod *et al.* (4) showed that even when the total peripheral resistance remains unchanged during emotional stress, the resistance may increase in some regional circuits (kidney and splanchnic area) and decrease in others (muscle). Renal vasoconstriction outlasted dilation in muscle and continued as a residual effect after the stimulus was terminated. In any case it has been amply demonstrated, at least on a statistical, actuarial basis, that transiently high pressures, whatever the underlying hemodynamics, are prognostic of the later development of hypertension, a fact which must be taken into account in a complete description of its pathogenesis.

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PHENOMENOLOGY AND EXISTENTIALISM IN PSYCHIATRY: AN HISTORICAL ANALYSIS[1]

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HISTORICAL ANALYSIS

In setting out to trace the development of phenomenology and existentialism as they pertain to psychiatry, I do not use the term *development* to reflect the chronologic sequence of publications and authors who in the march of time made contributions to that body of knowledge known today as phenomenology and existentialism and used for better understanding and treatment of mentally disturbed individuals. Instead of retracing this history as a sequence of steps made by different authors at different times, in different places, with different intentions and in different intellectual and cultural climates, I shall try to trace the march of reasonings implied in the two schools of thought in the light of medical history and medical thought. To accomplish this goal I shall use basic concepts, and try to derive from these foundations the special formulas and varieties of phenomenology and existentialism in psychiatry. Development thus understood is not symbolized by an unbroken line of events; it can best be illustrated by Goethe's idea of metamorphosis which, in its authentic version, conceives of each final product or part of a living organism as no more than a variation of the same basic type undergoing regional variations, according to time, places, seasons and functional requirements.

The reader who questions the legitimacy or usefulness of the historical considerations submitted in this paper is referred to the following significant passage of Nietzsche's essay, "On the Advantage and Disadvantage of History for Life" (*Vom Nutzen und Nachteil der Historie für das Leben*):

> "That life needs the service of history must be recognized with the same clarity as the statement ... that a superabundance of history is to the detriment of life. History belongs to life in a threefold sense: it belongs to life as it does to the one who is active and striving, as to the one who is preserving and venerating, and as to the one who is suffering and in need of liberation. Three kinds of history correspond to these three sorts of relationships: inasmuch as it is allowed to distinguish a *monumental*, an *antiquated* and a *critical* kind of history."[3]

Should the medical historian trying to trace the history and pre-history of phenomenology be requested to produce still more powerful letters of credential, he could call on a great medical historian of the past, K. Sprengel (29). In his little-known correspondence on Galen's philosophic system Sprengel cited Arcesilaus, the founder of the Middle Academy, as the inventor of the "epoche" (which was to become the crucial concept of pure phenomenology) or the "suspension of judgment"; Arcesilaus recommended extending this suspension to all sensual objects.

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[3] "Dass das Leben aber den Dienst der Historie brauche, muss ebenso deutlich begriffen werden als der Satz, der später zu beweisen sein wird—dass ein Übermass der Historie dem Lebendigen schade. In dreierlei Hinsicht gehört die Historie dem Lebendigen: sie gehört ihm als dem Tätigen und Strebenden, ihm als dem Bewahrenden und Verehrenden, ihm als dem Leidenden und der Befreiung Bedürftigen. Dieser Dreiheit von Beziehungen entspricht eine Dreiheit von Arten der Historie: sofern es erlaubt ist, eine *monumentalische*, eine *antiquarische* und eine *kritische* Art der Historie zu unterscheiden" (Friedrich Nietzsche, Erster Band, München (Verlag Carl Hanser), p. 219).

THE CRUCIAL CONCEPT

The medical historian who admits to being neither a phenomenologist nor an existentialist is in dire need of a definition of the authentic meaning and the true substance of the two terms, phenomenology and existentialism. But it has become increasingly difficult to know which, in our day, lies hidden behind a rather profuse use of the terms, not only in scientific publications but also increasingly in newspapers and bestsellers, on the stage and in the movies.

In this intellectual emergency the medical historian goes back to the origins, trusting that they may reveal the true and as yet undistorted meaning of a given doctrine threatened with loss of its genuine significance because of conflicting interpretations, prejudiced applications and polemics. In the case under discussion, the medical historian will turn to Husserl. He will then learn that the basic concept of pure phenomenology is that of the *epoche*. The crucial passage reads as follows:

> *"We put out of action the general thesis which belongs to the essence of the natural standpoint,* we place in brackets whatever it includes respecting the nature of Being: *this entire natural world therefore* which is continually 'there for us,' 'present to our hand,' and will ever remain there, is a 'fact-world' of which we continue to be conscious, even though it pleases us to put it in brackets" (12, p. 110).

The historian of medical ideas is tempted to consider the epoche as an experiment—made, it is true, by man's mind rather than by his hands. In two passages of his *Critique of Pure Reason*, Kant made thought experiments which remind us of Husserl's epoche. Indeed, to prove the *a priori* character of spatial intuition, Kant temporarily suspended in his mind all spatial objects and still retained pure space as the lasting condition or form of sensual receptivity. Similarly, when discussing man's freedom as consistent with man's natural or physical condition, he made it clear that, when blaming a given individual for his deed, we totally disregard or *set out of action* all empirical conditions and constitutional, social and biographic factors, and thus consider the individual as the true author or sole instigator of his action.

The epoche is the only "experimental" thought I can discover in the logical structure of phenomenology. The experimental method in its authentic version (Claude Bernard) aims at testing the truth of a preconceived idea or the immediate cause of a given phenomenon by the repetition at will of the same phenomenon. This implies history (i.e., past experiences) and causality, both disconnected by the phenomenologic reduction and both thrown into the background by the existentially minded individual, who thrusts himself toward an unexplored and unpredictable though widely open future. It is for the same reason that we cannot expect prognosis to be a major constituent of phenomenologic and existential thought in medicine. Prognosis is an experimental anticipation of the future which we reach from knowledge of the past and from present data. Since diagnosis also plays but a minor role in the two doctrines, therapy finally emerges as the sole major step which the phenomenologically or existentially minded physician allows himself to take.

With the "setting out of action," "disconnecting" or "bracketing" of the natural world and, subsequently, all of the natural sciences, little if any room seems left to the medical historian.

But the difficult situation which thus results from a faithful interpretation of pure phenomenology, and the apparently obstructive effect of the epoche on the exploration of natural phenomena, can in fact be eased. As Binswanger (2–4) states, the concept of phenomenology has undergone more than one change since the early days of the doctrine. We are taught that we must now distinguish between pure and empirical phenomenology, the latter a phenomenologic interpretation of forms of being or of

human existence. The crucial concept of Binswanger's analysis of man's being in the world is that of a design which must be conceived of as preceding even the earliest stages of human development, and which makes these early stages—or, in psychoanalytic terms, the history of the libido—possible and intelligible. Such a design may be a permanent striving for the *continuity* of life and the avoidance of discontinuities and sudden changes. The pathogenic effect of a separation from the mother is said to presuppose this basic striving for continuity, and for the avoidance of shocks which cannot thus be explained in their turn as the *results* of an isolation from the mother. The normal individual has more than just one design of life, but if an individual is limited to a single design, and this is threatened, anxiety results. In this view, the design of an individual's being in the world indeed precedes his history; in a sense, the basic concept of pure phenomenology—*i.e.*, the "epoche" or the suspension of objective reality or, to say the least, the *a priori* component of human existence—seems to be saved.

But it still remains true that the manifestations of mental diseases belong to the natural order of things. Ever since the nineteenth century, when psychiatry was shifted to that division of human knowledge which has become increasingly well known as natural sciences, any metaphysical or theologic interpretation of mental disease seems irrevocably outdated. At first approach, no room seemed left for an interpretation of mental disease as rooted in phenomenology, in which the basic point of departure is precisely the keeping in suspense—or bracketing—of the natural order of things. But in a second sense, phenomenologic writers remained faithful to the spirit of the phenomenologic reduction, or epoche. The most striking and least deniable constituent of man's natural existence is his body. A major part of the history of psychiatry has been written in terms of physical concepts of mental disease and treatments based on them. But psychiatric writers trained in phenomenologic thought and method never refer to man's physical equipment and processes as ultimate sources of their interpretations and explanations. This self-inflicted neglect can best be illustrated by the following passage (to be found in a fragment of Jasper's autobiography):

> "... As a physician and psychiatrist I saw the precarious foundation of so many statements and actions, and beheld the reign of imagined insights, *e.g.* the causation of all mental illnesses by brain processes (I called all this talk about the brain, as it was fashionable then, brain mythology ...)" (13).

But one should know in positive terms the method used by the writers of this school of thought. The diseased individual *in toto*, his particular mode of being or his specific existence, is declared to be the end at which investigations are aimed. It thus becomes intelligible that the authors adopting this goal deliberately leave behind the analysis of isolated functions and acts, but also to some extent the psychoanalytic doctrine of the libido and the isolated impulses on which this doctrine allegedly rests; similarly, the phenomenologists discard the analysis of constitutional types and, of course, all those neurophysiologic constructions called forth to explain the obsessive impulses not infrequently seen in postencephalitic patients. In most simplified but still authentic terms, the method used by phenomenologists can be called a descriptive one. Rümke, an eminent Dutch psychiatrist, using the phenomenologic method, defines the latter as follows: "Describe with the greatest possible precision the subjective experiences of the patient ..." (26). But this statement needs qualification.

In the natural sciences, but also in clinical medicine, descriptions are believed to be faithful if they convey to the listener or the reader a complete picture of all constituents of a given morphologic structure or physiologic process; in the latter case

(*i.e.*, that of a process), they imply causal thinking. It is generally understood that no disease process is fully described until its etiology is known—but etiology is the science of pathogenic causes. In modern psychology and psychopathology, causal thought assumes the form of genetic thought, a given life situation or character type being traced back to its sources or origins in infancy or ancestry. We are taught that understanding is reached after total reconstruction of the chain of determining events, though this reconstruction can never be considered to be an integral one. But neither the breaking up of a given syndrome into a maximum of constituent fragments nor the causal nor the genetic thinking is a carrier of phenomenologic descriptions. In phenomenologic and existential descriptions, ideas, feelings, types of behavior and conduct are not simply described in terms of their fragments, nor are they listed as the results of a genetic process; ample use is made of analogies and metaphors.

ANALOGIES AND AMBIGUITIES

This was not the first time that thinking in terms of analogies appeared in the history of medical thought. In fact, the ancients and the men of the Renaissance indulged in this:

> "Plato imagined the whole world as a single visible living creature. Similarly, Leonardo ascribed to the earth a spirit of growth and called the soil its flesh; its bones are the successive strata of the rocks which form the mountains; its cartilage is the tufa stone; its blood the springs of its waters . . ." (14, 22).

In the Hippocratic and Galenic traditions the heart was believed to be the seat of the innate heat, an analogy being drawn between this true hearth of the organism and the place where the sacred fire was burning in every Greek or Roman home (15).

In phenomenologic writings this mode of thinking is being cultivated as never before in medical history. Here are the sources of an unmistakably poetic style and expression which cannot be denied to many a phenomenologic and existential author, and which will not disturb those readers whose insights into the phenomena of life and death, mental health and mental disease are not geared to the traditional and sharp demarcation of science from art. It is true, however, that a certain ambiguity must result from the use of the same term to indicate two entirely different spheres of meaning, one physiologic or spatial, the other purely mental.

To cite a significant example: Is the advancing movement of a pedestrian not basically different from that of human life or of ideas? Is the human expansion, to cite another example, not basically different from the propagation of odors and perfumes (16, 17, 18)? These differences will not disappear unless one liquidates the Cartesian position, from which, nevertheless, pure phenomenology took its start under the guise and the title of *Cartesian Meditations*. What remains as the identical ground which encompasses the physical and the psychic is called, by the writers of this school of thought, the "existential." It seems that we have reached the ontologic root of the whole doctrine.

However, not every author of this school of thought goes so far as to identify the two series of phenomena. Von Gebsattel (8–11), believing in an existential void as the foundation of the manic-depressive form of existence, nevertheless interprets the utterances of melancholics as images, metaphors or parables. Another critical situation must emerge when the image used as an analogy is in its turn used as a springboard for the clarification of the original idea or symptom. In the technical terms of an eminent author (E. Minkowski) using this method, an existential component cannot be denied to the purely motor aspect of certain activities. This existential component even admits of a differentiation of these activities. We thus must be prepared to learn that the distinction between the literal meaning and

the figurative one of a given term ceases to have any significance for the phenomenologist. The reader might ask whether there would be danger in liquidating this distinction, and in referring both the literal and the figurative meaning to a primordial structure, or to the common existential root of the two terms of an analogy or a metaphor. Indeed, to relinquish the figurative meaning by dissolving it in a common existential root might deprive human language of one of its most powerful and eloquent means of expression, and thus impoverish one of man's most significant and noble endowments, *i.e.*, his language.

But we are taught by the phenomenologists that such expressions as the "fall" of man, "high" spirits, "low" morals are not mere analogies in which the physical or spatial qualities of high or low serve as models for psychic ones. The age-old alternative between mind and body is denounced, and the expressions just quoted are interpreted as significant criteria of the ontologic structure of human existence. But with the liquidation of an interrelation of mind and body, the body ceases to be the expression of man's inner life, thoughts and emotions; moreover, the alternatives between outer and inner life—in brief, between subject and object—have lost all of their meanings. In the light of these views, more than one of the most significant chapters of medical history have come to an end—from the ancient and now obsolete doctrine of the seat of the soul, to its modern version, *i.e.*, the doctrine of cerebral localization.

A 19TH CENTURY FORERUNNER

Moreau de Tours was the third link in a genealogy of French alienists that started with Pinel, extended to his pupil Esquirol (6), finally to reach Moreau de Tours who, in his turn, was a pupil of Esquirol. Moreau de Tours' treatise on hashish and mental alienation is most timely because of the careful auto-analysis made in a state of experimental intoxication, thus anticipating results obtained in our own days with mescaline and other drugs. Moreau de Tours was in close contact with contemporary writers, novelists and poets belonging to the romantic movement. He was not unimpressed by the latter, and quotes Gérard de Nerval (whose name was recently and justly revived by H. Ellenberger [5] to demonstrate the impact of early self-description of mental conditions on psychiatry and the ultimate rise of a phenomenologic approach). Gérard de Nerval was one of the foremost representatives of the French romantic school. Here may be found some minor historical sources of phenomenology and existentialism in which we may identify romantic elements, such as the emphasis on subjective experience, the denial—or at least the depreciation—of objective reality or the natural order of things, and, above all, the majestic role ascribed to death in both romanticism and existentialism. Romantic elements have also been uncovered in psychoanalysis, especially the longing for a happy past, and the decisive role left to the passions and drives at the expense of the classical ideal of reason. One is tempted to conclude that neither of these great doctrines denies romantic vestiges (though each does so in its own terms).

But Moreau de Tours went far beyond the romantic glorification of dreams and insanity:

> "... The alienist must follow, he [Moreau] said, as closely as possible the changes that take place in the inner existence of the insane. In order to penetrate still further into the incipient disease and follow not only its external and perceptible development, but also its internal evolution in the very depths of the consciousness, we must be able to study it on ourselves; or else, even though it may be infinitely less reliable, secure our information from the patients themselves ...
>
> "In our opinion, he concluded, mental alienation is a separate mode of existence, a type of inner life whose constituents and materials have necessarily been gathered in the real or posi-

tive world which it merely reflects and echoes. Dreaming is its most complete expression; we might say that it represents its normal or physiological equivalent" (7, 24).

I submit that Moreau's recommendation to substitute the expression *hallucinatory state* for that of hallucination can be considered as another anticipation of phenomenologic views in psychiatry:

"Hallucination ... involves all the faculties of the mind and is limited only by the barriers that nature has set to their activity; in other words, all the intellectual powers can be subject to hallucination, and not only one or another of these powers, for instance those concerned with the perception of sounds, or images, etc.

"It is for that reason that we can properly speak of a *hallucinatory state* and not of *hallucinations*. We shall use this latter expression, however, as have all other authorities; but it will indicate nothing more than purely accidental phenomena resulting from a general modification of the intellectual faculties.

"The hallucinatory state is the result of a *basic factor* which is the common source of all the abnormalities of the mind. It is a phenomenon of inner experience ..." (19, p. 168, 24, 25)[4].

DE-EMPHASIS OF DIAGNOSIS AND CLASSIFICATION

In the final analysis, the problems of diagnosis and of symptomatology are at stake. Phenomenologists confess to being less interested in raising the question of whether a patient suffers from schizophrenia than in trying to find an answer to the question, "What *is* a schizophrenic?" The question thus formulated leads them to the fundamental problems of being at large, and that of a schizophrenic patient in particular. The historian is tempted to see the same line of thought leading from Moreau's hallucinatory state to E. Minkowski's thesis, according to which psychopathology springs not from the study of symptoms but from that of various mental backgrounds and their significant structures. Implied in these and similar statements is an undeniable depreciation of the basic position left to the symptoms in any classificatory scheme in psychiatry and in clinical medicine of the past. But I do not know whether the impact of phenomenology on the various problems implied in classifications has ever been discussed by phenomenologists. At any rate, it has become a current theme of contemporary medicine to relinquish the once dominant role of isolated symptoms. Ever since the British school of experimental neurophysiology introduced the term *integration*, there has been a need to look at an apparently isolated activity of the living organism as the regional result and the embodiment of the activity of the whole.

I submit that, in phenomenology, only a very minor role is left to classification, *i.e.*, to generalization. In fact, the more individualistic an author's approach, the less interested he is in classification. Allowance must be made, however, for new entities or subtypes of nervous diseases as they appear in the writings of some existential psychiatrists. Such are the so-called *existential crises*, the *existential neurosis*, or the *noogenic neurosis* (Victor E. Frankl), the significant criterion of which is described as an individual's total loss of faith in the meaning of human existence.

Classifications are the results of generalizing thought which is resisted and therefore limited by the individual. The tendency to describe mental disorders in terms of specific modes of existence is by its very nature a strictly individualistic one. The method has also been called a subjective one. According to Jean Wahl, an historian of existentialism, "It is only in subjectivity that one may know existence, not in objectivity" (34).

[4] He reaches a similar interpretation of *delusion*: "A delusion, however simple it appears to be and whatever rational aspect it may assume, can only be the result of a profound and radical modification of the intellect, of a general disruption of our faculties. It is indicative of a total transformation of the thinking being, at least within the limits of a given chain of ideas. It has, especially recently, been confused with error. This is a serious mistake, psychologically speaking. A madman is not in a state of error. He acts logically in a sphere essentially different from ours...."

As a subjective method, this school of thought must leave dissatisfied those readers who cherish the idea of science as a body of factual evidences that can be repeated at will and thus demonstrated objectively to the eyes of any observer or experimenter, provided conditions remain unchanged. The latter qualification marks the vulnerable area in this concept of science, conditions in life never remaining unchanged. I presume that the whole question of whether one should ascribe or deny to individualizing thought the criterion of science might be a problem of secondary importance to the phenomenologist, provided his method promises the desired insight into the modes of existence of his patients. We thus understand the unusual leeway for individual observations in the phenomenologic literature, many papers containing lengthy case histories, and some consisting of no more—but also no less—than a single case history. It is of course true that the traditional psychiatric textbooks and publications also convey case histories. But these are offered as no more than illustrations or instances of disease processes or entities, whereas in phenomenologic writings they appear as originals rather than illustrations, as *substances* rather than instances of the final formula.[5]

VESTIGES OF GALENISM?

By leaving the natural order of things in suspense, pure phenomenology not only denies itself any place in medical history, but also, in a sense, even reverses the traditional order followed by the physiologist in his effort to explore the empirical reality. We are accustomed and trained to derive our sensations from the structures and the activities of our sense organs, which thus appear to be the origins or birthplaces of our senses. But in phenomenologic thought, sensations appear as the first and not the last links in this chain of events, as immediate manifestations of life, calling, in turn, for specific organs destined to reveal in a specific way our experiential world and the scope of each sense. Thus we reach an understanding of von Gebsattel's (8–11) statement that the experience of the void significant of certain types of melancholia *precedes* the activities of the senses.

In a similar fashion, E. Straus (31–33), discussing the dance, contrasts the traditional scheme of an intentional motor act, as a sequence of directed movements succeeding a given stimulus, with a nondirected and nonlimited type of movement, omnipresent throughout the dance. Thus it is not a chronologic or historical process, or its diagrammatic representation, but the immediacy of experience which the author tries to capture in his interpretations of perception and movement. Though these interpretations are not in line with traditional teaching, they mark one of the few areas in phenomenology where the medical historian is on firm ground. It is precisely this approach which is significant of Galen's physiology, as outlined in his famous treatise on the natural faculties. By the reversed order of things, Galen as well as the phenomenologist may justly claim to address the individual directly in his living state and his totality, thus bypassing the number and position of the manifold structures and processes of which we have indeed no immediate knowledge. It is tempting to surmise that here we reach one of the areas in phenomenology from which existentialism branches off, at least in its physiologic implications. But I must confess that, in spite of H. Ellenberger's (5) discriminative and most helpful analysis, I still experience dif-

[5] One is not surprised to learn that as early as 1845 the same Moreau de Tours listed auto-observations made under the influence of hashish, in which the perceptions of space and time were profoundly altered. Experiences of this kind are favorite themes in phenomenologic literature. Moreau reported the observation made under the same experimental conditions by the famous French writer Th. de Gautier, who believed he had lived 300 years, whereas in reality the crucial period lasted only 15 minutes. Moreau referred here to certain states of mental alienation, in which patients believe that they are a hundred or a thousand years old, and some think they are eternal.

ficulty in keeping both fields apart. With this restriction in mind, I submit that in simplified terms existentialism, particularly its French version, is reached as soon as we pass from the sole *interpretation* of vital processes to the mastery of human life and its requirements. Since antiquity, man has been thought to undergo—passively, if not helplessly—impressions, sensations, accidents, injuries and diseases. In other words, one generation after another has stressed the passive attitude of man in those basic processes and events from which he seems to emerge as the pure target or the victim. The reader of existential literature—again, the French version particularly—is impressed by the surprisingly small area remaining for the determining role of all external events—in brief, to the small or nonexistent area remaining for what the Greek philosophers and physicians meant by *pathos*. In contrast, it is the verb *to make*[6] which is in the foreground of the vocabulary of the existential school of thought. According to the teaching of this school, each man is not primarily endowed with a character, but makes himself what he actually is, and only after he has acted in a certain way and direction does he assume the type of human figure for which he stands in our eyes. This helps us to understand why one of the most eminent French existential writers (Simone de Beauvoir) (1) prefixed one of her treatises with the following quotation from Montaigne: "Life is neither good nor bad in itself, it is the place of good and evil according to what you make it . . ."

It is not by chance that the author selected Montaigne as her intellectual patron. Montaigne still embodies one of the significant constituents of the intellectual structure of France, *i.e.*, its skepticism. But a skeptical element cannot be denied to that version of existentialism which has no trust in ready-made values and abstract laws conceived and formulated in isolation from man's living state or individual existence.

MORAL IMPLICATIONS

Not for the first time in the history of psychiatry was the concept of *responsibility* allowed to be a major constituent in the therapeutic devices intended to impress and to educate the mentally disturbed patient. Brief reference must be made to those patients who, in the Middle Ages and during and even after the Renaissance, were afflicted by hallucinatory confusion, anxiety and delusions for which they were held responsible and treated accordingly.

When, toward the end of the eighteenth century, Pinel reshaped the human figure of the mentally disturbed individual, he was most anxious to liberate him from the physical and intellectual fetters of prejudice and superstition. Nevertheless, he retained the concept of responsibility, though from then on the patient was summoned to stand trial before his own conscience and his social commitments, rather than before the jury of the Inquisition and its unavowed successors. Although, contrary to the German alienists of the Romantic period, Pinel did not intend to judge and to treat his patients morally, he called his treatment *traitement moral*. The term has been frequently misunderstood, due to misinterpretation of the French word *moral*, the meaning of which is not restricted to a purely ethical connotation, but more precisely implies certain emotional and behavioral attitudes, an individual's "spirits" and, more generally, his

[6] "To the existentialist there is no other love than that which is being established, no other possibility of love than that which manifests itself through a given love; there is no other genius than that which is expressed in his works. A man engages himself in life and traces his figure; but besides this figure there is nothing . . . through his dreams, his expectations and hopes an individual can be defined only as an unfulfilled dream, as abortive hopes and useless expectations . . . man is nothing but a series of undertakings, the sum, organization and whole of the relations which constitute those undertakings. Repeatedly, the moral action is compared with an artistic product, which takes shape while it is being made. Similarly, man who is not shaped at the onset shapes himself by choosing his morality" (27).

psychic equipment. The best translation of *traitement moral* would seem to be "psychotherapy." In this respect, Pinel's treatment can be considered as an anticipation and a first outline of a psychotherapy of psychoses. In our own time, the late Dr. Frieda Fromm-Reichmann was eminent as one of the first and boldest apologists for intensive psychotherapy of psychosis (21). In advocating self-realization as the ultimate goal of psychotherapy, she was not too far from existential principles. Fromm-Reichmann, however, tended to stay within the limits of psychoanalysis, in the authentic texts of which the concept of responsibility does not occur. Unquestionably a fundamental difference between psychoanalysis and existentialism remains, in that in psychoanalysis man emerges as the *final* link of a genetic process or a historical development, whereas in existentialism man emerges as the creator of his own biography, for which he is requested to claim the sole and first authorship. I therefore wonder whether the frequently used term, existential psychoanalysis, may not lead to confusion.[7]

Of all versions of existentialism, the French one in its first setting is intended to prepare most forcefully a new design of human conduct. It seems that the purely negative or even destructive constituent in Sartre's existentialism should be conceived of and evaluated as a mere prelude to a new, more sincere and more "authentic" way of life; it is this call for greater truth and sincerity which marks the moral element of French existentialism, and thereby challenges the psychiatrist anxious to open to his patient new and safer ways of life and communication. It is by the same constituent that French existentialism seems to continue a line of authors who could be called *unmasking psychologists.* Up to the nineteenth century, France had contributed more than any other nation to this school of thought, which leads from Montaigne to Pascal, la Rochefoucauld and Jean Jacques Rousseau. In the nineteenth century there was a shift toward the German-speaking nations, and the names of Nietzsche and Freud began to dominate the scene of man's attempts to unmask himself and his institutions.

We are taught by the French existentialists that man has lost the immediacy of contact with life in all its spheres. Between him and his fellow human beings, as well as between him and his objects, a body of fictions and abstractions has originated, the result of tradition, education, ideals and science, separating man from both his fellow human beings and his objects. Experiences take place according to psychologic, social and moral rules, isolating man from the true sources of life and depriving him of the experience of immediate, unbroken and untranslated joy and misery. We live our ideas of human life and its values, rather than life itself, the undiluted sources of which we no longer reach. It is from this viewpoint that we are able to understand and to tolerate the experiments and experiences of so many of the figures in Sartre's novels, even the most primitive, brutal and seemingly abject and obscene ones: they were created for the purpose of rediscovering the pure sources of life in all its spheres. From the same viewpoint we can understand Sartre's objection to all attempts to explain life—in brief, to psychology, implying, as it does, reflection and analysis. We learn that the road to the rediscovery of

[7] Though Sartre himself was the first to use the term, he was anxious to distinguish his existential analysis from empirical psychoanalysis. The ultimate term of existential psychoanalysis is defined by Sartre as a *choice.* It thereby renounces the supposition that the environment acts mechanically on the subject under consideration. The environment can act on the subject only to the exact extent that he comprehends it—that is, transforms it into a situation; hence, no objective description of this environment could be of any use. The following warning implies a still greater remoteness from empirical psychoanalysis. The investigator, he says, must recall on every occasion that his object is not a datum buried in the darkness of the unconscious, but a free, conscious determination (28).

the sources of life must lead to the drama of life, and to the stage where man is acting but not merely interpreting life and himself (27).[8]

An attempt will now be made to assign to these views their place in the history of those ideas incorporated by contemporary medicine.

Diagnosis is one of the oldest medical functions. At times it was given an overall importance at the expense of interrogation, therapy and prognosis. Indeed, in the second half of the nineteenth century, diagnosis and classification seemed to usurp the physician's entire effort, and to emerge as the only major steps taken by the physician and taught to the student. The twentieth century brought a significant change. The new tendencies focused on the diseased individual, and threw into the background the name of his disease, its diagnosis and classification. This new school of thought gained great momentum in psychiatry, which had developed the greatest need for it. The question was no longer, How should the patient's condition be named and grouped (the name automatically calling for therapy and prognosis)? The question now was, What goes on in the patient? Which are the causal dynamics at work, regardless of diagnosis and classification? It was logical that the term *analysis*, as old as controlled human thought, was adopted for this type of inquiry which, from the historical as well as the epistemologic viewpoint, remained intimately bound up with the philosophic tendencies of nineteenth century medicine—above all, with the experimental method by which the physiologist and the experimental therapist, interrogating nature, try to reach the physiopathologic mechanisms at work and to act according to causal indication. Through the exploration of the causal dynamics, this school of thought was bound to look at the past and to conceive the actual condition of the patient as the final stage and the result of his biography. In brief, this school of thought was not only psychologic but also thoroughly historical.

But the development and growth of contemporary thought in medicine, or at least in psychiatry, have progressed still further. Deprived of causal dynamics and interpretations of symptoms by disturbed psychophysiologic mechanisms, and renouncing history, investigation turned to the patient's inner experiences in their true and living states, disregarding diagnosis as well as classification, and even the history of the patient's drives as well as the psychologic laws presiding over his behavior. The unmasking tendencies which existentialism has in common with psychoanalysis and its forerunners testified to a considerable psychologic factor which, apparently, existentialism also did not want to renounce entirely. The major problem, however, remained that of the world of the sick individual, his mode of existence, his communication with the world and with himself.

It is the failure to reach an absolute decision, or to make a choice between conflicting issues, which is believed by phenomenologists to be at the root of certain states of inner unrest and disrupted personalities, of mental alienation and, above all,

[8] The existentialist does not understand the universality of man to be that of human nature common to all men thought to be in possession of the same basic qualities. We are taught that this view would mean that man's essence precedes his existence, or that an individual's concrete being would have to be considered as a particular instance or offspring of such abstract concepts as human nature. According to the existentialistic view, man exists and emerges before he can be defined by any concept. Man's existence, we hear, precedes his essence. Man is only what he wants to be and what he actually undertakes to become. Originally, man is a project that takes shape in a universe of his own. This is called existentialistic humanism. It seems to this writer that the unfortunate alternative constructed by Sartre between an anonymous or *abstract* "human nature" and the *concrete* and truly existing individual can be silenced by conceiving the latter as the living carrier or representative of the former, individual man as the trustee of mankind; in brief, by claiming, at least in principle, the identity of the two terms, *i.e.*, mankind and individual man. Such, at least, was the version of humanism reached in the eighteenth century.

of melancholia. The analysis of the manner in which time is experienced under such conditions is said to promise a deeper insight into the genesis of melancholia, in which the individual is said to be cut off from his own future[9] and is thus prevented from growing and realizing his potentialities. In other conditions, such as compulsory states or addictions, he is said to be condemned permanently to repeat and revive the past, with no hope of bringing it to an end, and to enter the future which, under these conditions, is experienced as shrunken. The patient thus eliminates himself from time, the only medium in which alone he can grow and reach self-affirmation.

The great importance which time and future play in phenomenologic and existential interpretations of human life and disease reminds the medical historian—and, more particularly, the historian of neurology—of similar views held by neurologists, dealing, it is true, with "natural man," and therefore far remote from phenomenologic research. That the time factor intervenes in the results of experimental stimulation was first shown by Sherrington and the British school of neurophysiologists. But it was left to C. von Monakow and the Swiss school to stress the two major stages of brain injuries and brain disease: those of initial and transient and those of residual and lasting symptoms. The time element is still more conspicuous in Monakow's "chronogenetic" localization, the basic element of which is as follows:

> "... Each nervous function is the result of a historical evolution. Von Monakow understood the finished function to be a highly evolved product arising from reflex mechanisms and finally becoming conscious motion and sensation. During this developmental process, a given function passes through various stages, all of which are believed to be but fragments of the function in its definite form, and all of which are related to the various structural stages of brain development. These constituent fragments of a given nervous function remain latent, but may be mobilized at any moment ..." (23).

At the final stage of his career Monakow, after having devoted more than 50 years of his life to empirical research, and under the influence of the profound shock produced by the outbreak of the First World War, felt the need to turn to those organogenetic factors which he believed presided over growth, maturation and regeneration. It is true that his material and his views were purely biologic ones, but he included the future of the individual and even that of mankind in his vitalistic scheme of the "giant protoplasma man." Nervous diseases, he said, may be due to an individual's betrayal of the future, which he (Monakow) believed to be an important component of the vital program inherent in even the smallest fragment of life. But he also included a biologic pre-stage of moral conscience (*syneidesis*) in his design of the living organism. This brings me back to a brief discussion of the relation of existentialism to ethics. In fact, the moral issue has been raised by more than one existential psychiatrist.

The medical historian cannot bypass this aspect of existentialism. One of the earliest documents of medical history, one that has withstood the test of more than 2,000 years, is the Hippocratic oath, which embodies the principles of medical ethics.

Kierkegaard's book, *Either-Or*, is undoubtedly a treatise on ethics, in which the author contrasts the esthetic way of life with the ethical one, the former being at the service of the present alone, the latter reaching out for the future, if not for eternity. Moreover, the terms so frequently occurring in existential writings—responsibility, original choice, absolute decision, guilt and freedom—have an unmistakably moral connotation.

Our times are no longer interested in the classification of human knowledge. But the encyclopedists of the eighteenth century would have shifted existentialism to moral philosophy, perhaps side by side with psy-

[9] Propulsion toward the future was totally lacking in one of E. Minkowski's schizophrenics.

choanalysis; though the latter, in its original and Freudian version, never intended to do more than to enlighten man as to the natural conditions or foundations of his behavior, without using those foundations for a design of human conduct, the criterion of the patient's final recovery of mental health being defined by Freud as his regained ability to make free use of his once-arrested libido. In contrast, existential writers impress their readers by a strongly educational attitude, and by their undeniable tendency to guide man and not merely to explore him. Room is left in this existential psychotherapy for the choice of the best possible moment for successful intervention. This rule revives the ancients' concept and form of crisis through which diseases were observed to pass.[10] One writer says, "The essence of each ethics is to consider human life as a game which one can win or lose and to teach man the means by which to win."

The quotation is from the only treatise on ethics produced so far by the contemporary existential school, Simone de Beauvoir's *Pour une Morale de l'Ambiguité* (1).

The recurrent theme of this book is man's freedom. Morality, the author says, is "the triumph of freedom over factuality" ("facticité") (1, II, p. 64). In other passages, freedom is simply identified with morality ("se vouloir moral et se vouloir libre, c'est une seule et même décision") (1, p. 35). Repeatedly, the autonomy of man's choice or decision is stressed. One is thus not surprised to learn that existential ethics, which sees in man's freedom the source of all values, does no more but also no less than to revive and to amplify the teachings of Kant, Fichte and Hegel. But, in contrast to Kant, it is neither anonymous nor universal man who emerges as the source of values; it is the plurality of concrete and particular individuals, who thrust themselves toward their goals, setting out from situations the individuality of which is as radical and irreducible as subjectivity itself (1, p. 26).[11]

Simone de Beauvoir draws still another line of demarcation between Kantian ethics and her own "moral of ambiguity," and at this point therapeutic implications are obvious. She interprets man as he emerges from Kant's ethics as endowed with an intrinsically positive will, or as in agreement with himself ("coïncidence avec lui-même") (1, p. 48); whereas, in existential ethics, man at his beginning is defined as a negativity at a distance from himself, this "being at a distance from himself" recurring frequently as a criterion of man's initial situation. It is because of this initially negative nature of man that existentialism, not unlike religion, leaves room for evil. Generally speaking, it is one of the significant features of existential ethics —at least of the French version here considered—to affirm but not to mask the contradictions, or "tragic ambiguities," which are the criteria of man's unique situation. One of these tragic ambiguities is marked by the dialectics between man's inwardness and the crushing power of external objects. Another ambiguity results from man's twofold nature: as a mind and as a body. Another qualification of existential ethics is implied in the repeatedly stressed refusal to establish as absolutes the goals toward which man thrusts himself; these goals should rather be considered in their union with the freedom or subjectivity from which they spring. A far-reaching expression of the same thought is embodied in the tenet that man bears the responsibility of a world

[10] The Greeks here affirmed their sense of tragedy.

[11] But I am not too sure that the author reached the authentic meaning of Kant's conception of man's moral nature. In fact, Kant never pretended to have furnished the dogmatic proof of man's freedom; he stated only that this freedom, or man's moral nature, can exist side by side with man's physical nature, provided one limits oneself to an interpretation of both man's moral and man's physical nature as two different *views* or two different modes of reasoning, according to situation. Far from claiming the proof of even one single moral act, Kant maintained no more than the intransigence of moral *thought*.

which is not the work of a foreign power but of his own, where his defeats and his victories leave their vestiges (1, p. 23). Again, the same thought appears in the refusal, so characteristic of existential ethics, to praise or to blame in the name of an abstract law. Existentialism here substitutes man's "subjectivity," this term also standing for autonomy, freedom, choice and individual conscience.

We would miss the full meaning of existential ethics were we to ignore the fact that man, in his striving for self-realization, reaches out for others. In technical terms, man's subjectivity transcends itself toward others; in more relaxed terms, no human existence can reach fulfillment or completion if it limits itself to itself; in still other terms, it is only in the existence of others that man can find justification for his own existence. But it marks yet another of man's ambiguities that he reaches out for humanity and still remains an isolated individual, involved in his own commitments and projects.

Here are a few examples of the impact of the moral component of existentialism on psychiatry. We learn that man is under the alternate but antagonistic influences of self-affirmative and self-destructive tendencies. The latter are said (by von Gebsattel) to be at the root of addictions, the ultimate sources of which are thus traced back to self-rejection and the resulting state of despair. Inasmuch as this self-rejection is grounded in a rejection of human values, an ethical component cannot be denied to these existential interpretations.

It is a trivial statement that guilt feelings are a part of the clinical picture of melancholia. It is an existential interpretation to derive these guilt feelings from the manner in which time is experienced in depressive states. This interpretation has been offered by von Gebsattel. Everyone's life is subject to the pressure of some guilt, from which we liberate ourselves by leaving it behind and, passing on to the future, become consecrated with a greater force and love. It is this passing on to the future which, according to our author, is impossible in melancholia. Guilt thus assumes its irrevocable, incorrigible and definite character.

Some existential psychiatrists, however, want to relinquish the moral connotation of guilt. Storch (30, p. 352)[12], referring to Kierkegaard, speaks of *existential* guilt; Rollo May, of *ontologic* guilt. Common to both is a concept of guilt arising from forfeiting one's own potentialities. Another form of ontologic guilt is said to arise from the failure fully to understand and to meet the other's needs. It is the latter form of ontologic guilt which best meets the requirements of ethics as a body of rules presiding over interhuman relationships, and which also seems compatible with the spirit and the letter of medical ethics. The Hippocratic physician taking the oath solemnly declares that he will abstain from whatever is deleterious and mischievous, and that, whatever houses he enters, he will go into them for the benefit of the sick.

Nothing is more difficult than to gain insight into the inner structure of existential therapy unless it is experienced or administered in its living state. In a sense it perpetuates or revives the Hippocratic principle, according to which "the patient must combat the disease along with the physician." Should it be true that the patient has recovered as soon as he has succeeded in reaching his freedom, absolute decisions and choice, then both patient and physician emerge as associates, rather than the one as the obedient target and the other as the autocratic director of treatment. I submit that this Hippocratic technical rule is an historic anticipation, though a very vague one, of the famous "encounter" said to mark the interrelation of physician and patient in

[12] According to Storch, the existential guilt lies in the *denial of being* (Guardini) in the sense of the non-acceptance of one's final allotted destiny (30).

existential psychotherapy. But, in another sense, at least one version of existential psychotherapy seems to break with age-old traditions in medical history. Ever since the days of the Greeks the aim of the healing art has been to alleviate suffering. But we now learn that the road to cure leads through renunciation, through the dismissal of the pleasure principle, and through suffering, which the patient must not only accept stoically but also affirm religiously,[13] to reach the meaning of human suffering and its ultimate blessing. Here it appears that it is not the patient but the physician himself who must assume responsibility for what is no longer an unqualified and quick relief, but rather a heavy demand made on a helpless or depressed individual requested to assume full responsibility for his entire life, inclusive of injury and disease. One is reminded of a saying of the Roman stoic Seneca:

> "Precepts by themselves are weak and, so to speak, rootless, if they be assigned to the parts and not to the whole. It is the doctrines which will strengthen and support us in peace and calm, which will include simultaneously the whole of life and the universe in its completeness."[14]

Thus is left on both patient and reader the impression of a stern atmosphere in which more seems to be at stake than just a person's well-being and mental health—namely, man's greatness, his place in human society and on earth. It is for this reason that the distinction between normal and abnormal ultimately disappears behind issues of much greater scope. Binswanger (4) defines the specific task left to an existential psychiatrist as the study of the changing structure of being in a given individual, regardless of the distinction between normal and abnormal. Again we are on historical ground, though it is not a physician but the same literary figure, Gérard de Nerval, to whom we can turn once more as to a forerunner. The opening chapter of de Nerval's major work, *Aurélia*, contains the following passage:

> "I am going to try to describe the impressions of a long illness which took place entirely within the mysteries of my soul; I do not know why I use the word 'illness,' for as far as my physical self was concerned, I never felt better. Sometimes I thought my strength and energy were doubled, I seemed to know everything, understand everything. My imagination gave me infinite delight. In recovering what men call reason, do I have to regret the loss of those joys? . . ."

FANTASY AND POETRY

Here let us re-examine my opening remarks on the epoche and its apparently forbidding effect on empirical investigations. It remains true that, through the "phenomenological reduction," the objective world is put out of action, and that only the "transcendental subjectivity" remains as the sole theme of pure phenomenology. It is also true that psychology in its traditional setting is thereby put out of action, the theme of psychology indeed being "natural man," his psychophysical equipment and reactions, all belonging to the objective reality which is suspended by the "phenomenological reduction." But an opening to psychologic research was still implied in Husserl's intention to secure to his new and pure psychology its material from a groping experiential analysis of empirical events as

[13] Here are the sources of the religious version of existentialism. After having rejected as comic an individual's effort to be the absolute, to forget that, however original he may be, he is "still a child of God, of his age, of his nation, of his family and friends," Kierkegaard turns from the "aesthetic" responsibility to the "ethical" one. It is a universal sinfulness which asserts itself in the sinner who has taken the universal sin upon himself and thereby attains a consolation which is rooted in deeper grounds than the "aesthetic" consolation one may derive from the weakness of human nature.

[14] It is in the same vein that Kierkegaard, speaking of a man who chooses himself in an ethical sense, recalls the passage of Scripture which speaks of giving an account for every idle word. In terms of his own, Kierkegaard states: "He who lives ethically, abolishes to a certain degree the distinction between the accidental and the essential, for he accepts himself, every inch of him as equally essential."

they appear in mental experiences, associated with a free modification of these experiences through *fantasy*. He felt that phenomenologic psychology could benefit by the contributions made by history and art, and still more by those made by poetry. It is true that poetry offers only imagination, but this far surpasses the results of our own fantasies by the originality of new creations, the wealth of detail, the completeness of motivation; moreover, through the suggestive power of artistic means of representation, imagination is transformed with greatest ease into perfectly clear fantasies, provided it is guided by intuition and intelligence. "Fiction" thus becomes a vital element of phenomenology, and the source from which it draws the knowledge of "eternal truths." Thus the historian seems justified when he proclaims Gérard de Nerval, poets and artists as informed witnesses of the problems and views under discussion.

A NEW CONCEPT OF DISEASE?

We have reached the point where the medical historian raises the final question, Is a new concept of disease emerging from the phenomenologic and existential investigations and interpretations? We are taught that the normal individual is characterized by a great number of projects or potential designs of existence. In contrast, disease is believed to manifest itself by a limitation of these projects. Disease thus emerges as a restricted way of life. The history of medicine can indeed be written in terms of those measures and methods intended to alleviate and to correct the natural and social effects of a patient's restricted life, measures and methods embodied in the age-old terms of care, hospitalization, rehabilitation and re-education, and emerging in the nineteenth century as systematic psychotherapy. Moreover, in the normal individual, the various projects of existence are said to be flexible. In contrast, disease manifests itself by a greater rigidity. It is of interest that the same term was used by an eminent neurologist, Kurt Goldstein, to characterize a basic disturbance resulting from destroying brain lesions, the brain-injured patient being unable to shift from one attitude to another. It may not be surprising that a point of contact is thus reached between the two fields (*i.e.*, psychiatry and neurology), the separation of which owes its origin to no more than increasing and confusing specialization, to didactic purposes, to competence if not to competition. Helpful and necessary as this separation appears to be at the present state of medical knowledge and medical education, the patient himself neither knows nor cares about it. He wants only to be understood and assisted, regardless of the departments to which he is shifted by our classifications. If I have correctly interpreted phenomenology and existentialism in the light of medical history, I would venture to state, in oversimplified and crude terms, that the main concern of those clinicians adhering to these two doctrines is to obtain an unprejudiced and faithful picture of the patient's inwardness of which the patient remains the sole authentic and competent witness, and to the happy change of which the patient himself has to make the greatest contribution, namely, his own free decisions.[15]

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[15] It remains to be seen whether the view of man's total conduct as the result of an original and conscious choice reflects a postulate rather than a demonstrable fact. It seems doubtful, to say the least, that all of man's actions bear the mark of original choice or conscious project, even if we grant that man has more than just one project. But should man never be endangered of becoming estranged to himself, should he always be free to choose and to act according to his own choice, his figure would be inflated and reach almost divine proportions. However, to derive man's actions from an organizing center, instead of to array them side by side as organized links of a chain succeeding and conditioning each other as causes and effects, remains a principle of research and interpretation of greatest value, and in strict conformity with the Aristotelian concept of an organism having life in all its parts.

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PSYCHIATRY AND PHILOSOPHY OF SCIENCE: I. CONCEPTUAL PROBLEMS IN PSYCHIATRY[1]

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THE IMPORTANCE OF CONCEPTUALIZATION IN THE "SPIRAL OF SCIENCE"

Scientific conceptualization includes the *selection* of the terms which stand for the data of observation and for abstract, non-observational concepts; the assignment of *meaning* to these terms and the communication of this meaning and the *modification* of meaning as new observations are made. Careful conceptualization is as important to the advancement of knowledge as is controlled observation. Errors in the use of language have effects as grave as errors made during laboratory or field study. The philosophy of science provides a framework which may be utilized to discover, understand and avoid some common errors of conceptualization. Within this framework we shall examine two important conceptual errors: 1) reification and the inflexible use of scientific terms; 2) the failure to specify the meaning of terms with a clarity and precision sufficient to enable independent workers to agree in their usage.

Conceptualization and observation constitute the two mutually dependent phases of science. Reid and Finesinger (37, p. 419) view the scientific process as a spiral in which investigators concentrate alternately on "the logical properties of the language used and on the empirical traits of the subject matter being investigated." With each turn of the spiral there is a return to these two phases, which we have called conceptualization and observation. The metaphor of the spiral is intended to depict the advancing nature of the process, which gradually provides more precise knowledge of increasingly wider areas of natural events.

Reichenbach (34, p. 231) recognizes the need to distinguish the phase of conceptualization from that of empirical testing when he speaks of the "context of discovery" and the "context of justification," adding that the act of discovery escapes logical analysis. Similarly, Meehl (31) refers to research as "a shuttling back and forth between clever, creative speculation and subsequent statistical testing of empirical derivations therefrom." The Committee on Research of the Group for the Advancement of Psychiatry (GAP) (15), in its recent report on controls in psychiatric research, includes discussions of the methodologic requirements of the conceptualizing as well as of the testing phase. The Committee emphasizes that, despite the importance of the "hunch" in conceptualizing, care in the choice of units of study and in the assessment and improvement of reliability of measurement is also of importance. This report points up the interdependence of the conceptualizing and testing phases. However, major emphasis is placed by the GAP Committee, as by most advocates of increased scientific rigor in psychiatry, on the reduction of error sources typical of the testing phase, *e.g.*, faulty sampling, observer bias, and unrecognized influence of the observer and social field upon the subject. The importance of attention to control of these variables in the design of research is now more generally recognized by psychiatrists who aspire to scientific status for their pro-

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fession. There is also, however, need for better "controls" during the phase of conceptualization.

This paper begins with a brief review of contributions demonstrating an awareness within psychiatry of the need for improved conceptual tools. An attempt will then be made to understand, within the framework of the philosophy of science, the two major errors of conceptualization in the hope that, thus located, these errors may be better understood and more easily avoided. In a second paper (19) a review of some of the recent attempts in psychiatry to reduce conceptual error will be followed by a discussion of an important but frequently neglected step in the process of conceptualization: the assessment of the intersubjective reliability of terms. It will be argued that the adequacy (meaningfulness) of the terms of a scientific language must be assessed by tests of reliability, and that efforts must be made to improve the adequacy of that language when it is found to be unsatisfactory.

RECOGNITION OF CONCEPTUAL PROBLEMS IN PSYCHIATRY

Some of the psychiatric literature of the last decade indicates a continuing, if not widespread, interest in conceptual problems. Benjamin (2), writing of psychiatric research, has deplored the general disinterest in classification, pointing out that in all natural sciences "there is no investigation without classification." Chodoff (6) terms diagnostic classification the King Charles's head of psychiatry, whose unsolved problem "mocks any pretensions that the psychiatric discipline is a scientific one." It is worthy of note that Chodoff's paper was read at a round-table discussion on the subject of meaningful diagnosis at the 1959 meeting of the American Psychiatric Association, and that a similar symposium was held in 1957 (17, 36, 41). In 1951 the American Psychopathological Association devoted its annual meeting to "Current Problems in Psychiatric Diagnosis". Among the papers from that meeting which deal with the more general problem of diagnostic concepts is that of Hunt, Wittson and Hunt (22), who point out the confusion and lack of reliability in diagnosis, attributing this lack largely to the "faulty diagnostic categories of a still young and developing science." Karl Menninger *et al.* (32) trace in detail the history of psychiatrists' dissatisfaction with their systems of classification.

Other papers direct attention to inadequacies in the conceptualization of particular disorders. The concept "schizophrenia" has been most sharply scrutinized. Benjamin (3) warns of the limited conclusions possible in studies "consisting of the comparison of a group of *undefined* schizophrenics with a group of *undefined* controls" (italics added). Szasz (41, 42) and Menninger *et al.* (32) have cited "schizophrenia" as the outstanding example of diagnostic terms which are harmful to practice and research, and they appear to advocate that the term be discarded. Worden (43), contributing a chapter to the book, *Schizophrenia: An Integrated Approach*, found himself unable to orient his presentation of neurophysiologic research around so nebulous a concept. Other diagnostic terms have also been criticized for their lack of conceptual clarity. Chodoff and Lyons (7) and, more recently, Berblinger (4) have written about the term "hysteria" and the "hysterical personality" from this point of view. Guttmacher's (16) review of "psychopathic personality" is an attempt to contribute to a solution of the semantic as well as the empirical problems surrounding the concept. Karpman (24–26) has also found it necessary to re-appraise the concept of the "psychopath."

Dissatisfaction with conceptualization is not limited to the official diagnostic terms of psychiatry. Robert Knight's (27) discussion of the one-sidedness of a character-diagnostic system based upon the Freudian theory of libidinal stages is relevant here,

as is his assertion that the old standby concept, "neurosis vs. psychosis," is conceptually inadequate. Diethelm (9) concludes that, because of the varied meanings given to the term "psychosis," it is a fallacious concept which "has dominated psychiatric thinking and facilitated loose and unscientific thinking."

Concern with problems of conceptualization extends, of course, beyond systems of nosology. The recent conferences at New York University (2) and at the New York Academy of Sciences (1), as well as the papers of Kardiner, Karush and Ovesey (23), serve as examples of the growing literature dealing with the concepts of higher-order psychiatric theory, particularly the theoretical terms of psychoanalysis. Hartmann (18) describes Freud as an outstanding example of "the two way approach whose work exemplifies not only the importance of observation for theory but also awareness of the role of good concept formation and good theory" for discovering facts as well as integrating them.

THE TWO MAJOR ERRORS OF CONCEPTUALIZATION: INFLEXIBILITY AND UNRELIABILITY

Modern philosophy of science may provide a useful vantage point from which to consider some common problems of the phase of conceptualization. It has been noted that this phase involves an element of creativeness, speculation and intuition for which no rules can be specified. However, two equally essential aspects of the process of conceptualization may be described in addition to the creative. The first involves flexibility in the use of concepts and theory, *i.e.*, a basic conviction that the concepts of a scientific theory are conveniences to be used for the prediction and understanding of observations, constantly modified in the light of new observations and eventually discarded when no longer useful. It is recognized that the postulates of such theories are never final truths, but are subject continually to further confirmation or disconfirmation. In the discussion which follows, we call the errors which tend to cluster around (and obscure) these principles of flexibility the errors of *reification*, *rigidity* and *narrowness*.

The second essential element of conceptualization involves the clear recognition that concepts must be so created or modified as to be plainly related to publicly observable objects and events, *i.e.*, empirically meaningful, adequate in this respect for use in subsequent observational tests of the postulates in which they occur. The measure of the empirical meaningfulness of a term is the consistency (reliability) with which it is used. Errors involving failure of agreement in the use of terms are called *unreliability*.

REIFICATION AND RELATED ERRORS OF INFLEXIBILITY

Mandler and Kessen (30, pp. 14–15) identify the error of *reification* as "the archaic notion that everything with a name exists 'out there,'" and they explain its prevalence by reference to the observation that language is first acquired through the naming of the objects of sensory experience. The error is widespread. Reification is implicit in the question, "What *is* schizophrenia?"; in the demand for the *correct* definition of "ego"; and even in the statement that "libido" is an unacceptable concept because there is no such *thing* as libido, the implication being that existence, in an ontologic sense, is a requirement of scientific concepts.

From a psychodynamic point of view, reification may represent an attempt to find security in certainty of knowledge. It is doubtless comforting to believe that one's concepts represent real and unchanging entities in the world "out there" (36, 39). More relevant to our purposes than hypotheses about its psychologic sources, however, are the consequences of reification, conse-

quences best understood by a further exploration of the nature of conceptualization.

Mandler and Kessen (30, pp. 14–15) continue their discussion of reification by relating it to the failure to distinguish between the observation and theoretical languages: "... names like 'mind' or 'force' are used ... as if their status were similar to that of such names as 'red,' or 'green' or 'hot.'" That is, in their view the error lies in mistaking theoretical for observation terms. We strongly agree that this mistake is to be avoided, but also wish to point out that even observation terms may be erroneously reified. There is a necessity, in empirical science, for a distinction between an observation language, in which words have their meaning as responses to non-verbal objects and events, and a theoretical language, in which meaning is acquired as responses to other words. However, in order to emphasize the need for flexibility in the use of words, we call attention to the artificial nature of *all* language. To reify is to ignore the fact that words are the products of man's creation. A flexible, non-reified attitude toward words and concepts permits the modification of language made necessary by new observations. There are thus a gradual change and development of the meaning of a term, not entirely creative or arbitrary but dependent in part upon the most recent empirical findings. For example, the finding of a close relationship between "general paresis" (behaviorally defined) and a positive cerebrospinal fluid serology results in an addition to the meaning of "general paresis." This process, called "empirical analysis" of terms (30, pp. 94–97), is focused on the recurring influence of new empirical findings upon the terms and concepts of a scientific theory. This influence depends upon the flexible use of terms. Reification is often associated with an inflexibility in the use of terms, an inability or unwillingness to revise the meaning of concepts during the recurring phases of conceptualization, an error of *rigidity* in the use of language.

Danto (8) attributes to reification of terms (which he calls "denotation") much of the difficulty experienced by a conference of philosophers and psychoanalysts trying to evaluate psychoanalytic theory. He writes: "Yet I think it is part of the psychoanalyst's commitment to believe in the existence of these *essentially* covert entities. And very frequently defenses of psychoanalytic theory are in fact no more than arguments in favor of the existence of these entities. So I think it would be intolerable to them to suggest that these might be only systematically required by a theory, the main purpose of which is to predict behavior. For it is, after all, the proud claim of psychoanalysis to have *discovered* the real mechanisms of the psyche ...

"But surely it is possible," Danto continues, "for a theory to be true independently of whether its theoretical terms denote.... But since psychoanalysts often identify the issues of adequacy and ontology, this might go a long way toward explaining why our discussion so frequently resembled a debate between believers and nonbelievers."

Needless to say, reification and rigidity are practices by no means limited to or universal among Freudian theorists. The terms of classical academic psychiatry, of the standard nomenclature or the mental status are often reified and rigidly retained.

Clearly, there are legitimate reasons to retain a concept. Science does not operate by creating a totally new set of concepts with every new experiment or observation. It is the scientist's prerogative to decide, in an act which is partially intuitive, whether at a given time recent findings and future goals warrant retention of concepts. But the inability to modify blocks the advance of a science.

Related to reification and rigidity is the error of *narrowness*, the failure to recognize

the necessity, especially in young sciences, for alternate classification and theoretical systems. In the early stages of science it is not possible to reconcile the many overlapping theories. If theories are viewed as convenient artifices, this need not lead to obsessive rumination or to dogmatic rejection of all but a single theory. One need not feel uneasy about employing one theoretical system for research and another in psychotherapeutic formulations. It is not necessary to demolish libido theory because one wishes to employ some alternative system. All of these profitless activities are likely to result from the need to maintain the illusion that one's concepts are the real and only correct way of looking at the data. There are, of course, legitimate empirical and heuristic grounds for favoring one theory and its concepts over another. Discussions which argue the confirmatory significance of new evidence for Postulate A as against Postulate B are to be encouraged. In the light of new evidence, the potential fruitfulness of Theory A over Theory B may be usefully debated. But arguments which favor a particular theory because it is thought to be the final picture of reality are not likely to be of much value.

THE ERROR OF UNRELIABILITY

The errors just discussed may be viewed collectively as an inflexibility in the use of language, the overall effect of which is obstruction of the creative step essential to the advancement of knowledge. We turn next to that group of errors which may be conceived of as unbridled creativeness, the careless production or adoption of terms and concepts without proper concern for their logically consistent anchoring in reliable observation terms, *i.e.*, terms demonstrated to bear a reasonably constant relationship to publicly observable phenomena. We have previously been discussing excessive rigidity of conceptualization; we turn now to excessive laxity.

Kubie (28) touches upon this area from the standpoint of the stresses which the scientific career places upon the scientist. He writes: "During the exploratory phase, while crude data are being gathered, an investigator ought to be free from rigidity. He should be ready to abandon preconceived objectives and anticipated goals, so that any hints that come from unexpected findings can be pursued. He must be psychologically free to follow uncharted courses. Therefore, premature systematization of the data must be avoided. This requires that type of free and imaginative flexibility which is sometimes attributed to the so-called 'hysterical' personality. Later, a more rigid process is required, one which has some of the features of the obsessional neurosis, or even some of the tendency of a paranoid patient to organize his delusions into logical systems. Scientific research thus seems to require that, as the work progresses, the investigator should be free to operate now with one type of personality and now with another."

The errors of loose conceptualization are termed unreliability because careful specification of the meaning of a concept is necessary for agreement in its use. Intersubjective reliability in the use of terms is a requisite of science. Feigl (10) writes: "Intersubjective testability . . . is only a more adequate formulation of what is generally meant by the 'objectivity' of science. What is here involved is not only the freedom from personal or cultural bias or partiality, but—even more fundamentally—the requirement that the knowledge claims of science be in principle capable of test on the part of any person properly equipped with intelligence and the technical devices of observation or experimentation. . . . The criterion of intersubjective testability . . . delimits the scientific from the non-scientific activities of man." The decision to admit as data of science only phenomena open to public inspection is vitiated if the language

used to communicate about these objects and events is not intersubjectively meaningful.

To discuss the adequacy of terms with respect to their objectivity or empirical meaningfulness, it is necessary to distinguish the theoretical language from the language of observation (5, pp. 38 ff.; 11, p. 20; 30, pp. 24–25, 50–53, 89). It is important to remember that observation terms are not fixed and final symbols, and are, with theoretical terms, liable to the errors of inflexibility: reification, rigidity and narrowness.

Reliability of Observation Terms: For observation words, the matter of objectivity is, in principle at least, relatively straightforward. The data to be encompassed by a science must be represented by a set of observation terms which can be reliably applied to those data, a set of terms which have been demonstrated to be intersubjectively consistent verbal responses to nonverbal objects and events. The basic requirement for any body of scientific knowledge is a set of observation terms the meaning of which can be agreed upon by those trained in the science. While simple in principle, objectivity in practice is often most difficult to achieve, especially in the sciences of human behavior.

Psychology has long recognized the special problems of objectivity which the complex nature of its subject matter creates. The historical lag in the development of psychology as an empirical science, when compared to the much earlier emergence of the physical and biologic sciences, may be partially due to the difficulty of specifying reliable observation categories. The concern of psychologists with this problem is reflected in the attention traditionally given to reliability when constructing tests and other instruments of behavioral measurement. Discussing the empirical basis of psychology, Kenneth Spence (40) writes: "I would propose to accept as the criterion for the selection of this basic vocabulary of psychology the methodological requirement that its terms designate a class of observations that display the highest possible degree of intersubjective consistency." Mandler and Kessen, whose excellent book on *The Language of Psychology* (30) has been cited previously, make repeated reference to this same requirement: "In the search for a basic vocabulary for psychology several classes of terms have been proposed and found wanting. The major criteria which have emerged are those of precision and reliability of usage" (p. 46). "The demand for precision and reliability may be interpreted as a demand for response invariance. Scientific terms must be used invariantly from scientist to scientist, and from occasion to occasion" (p. 56). But lest it be thought that Mandler and Kessen are armchair visionaries making totally unrealistic demands upon the working scientist, we quote a final passage: "Clarity, precision, and invariance can only be demanded as a goal of the scientific language. Invariance of a term can be developed, but the term or some approximation must be present in the first place. To restrict the scientist to the language available at any point in time would restrict his creativity. But to permit him to continue to use his new language loosely without reference to the scientific community would never bring his insights into the larger body of science. In this sense then, a balance must be found between unrestricted imagination and a necessarily restrictive channeling of its products" (p. 72). Failure to subscribe to even such a moderate demand constitutes the error of *unreliability*.

Psychiatrists, as a group, have been less concerned with control of these sources of error than have other behavioral scientists. The demands of therapy introduce additional complexity into any data-collecting situation. If, however, the clinical interview and the therapeutic relationship do indeed provide a unique opportunity for observing certain otherwise hidden aspects of human

activity, it is imperative that these observations be identified and conceptualized in language that will permit their full incorporation into psychologic science.

Reliability of Non-Observation Terms: Acceptable theoretical terms must be adequately anchored to other words, and their lines of anchoring must, at some points, come to rest in the observation language. As observation terms may be said to be directly object-and-event or nonverbally anchored, theoretical terms are directly anchored intraverbally and only indirectly to objects and events. Formerly, the single requirement that all theoretical terms be fully defined by or reducible to observation terms constituted an "empiricist criterion of meaning," violation of which was thought to place a concept outside science. This criterion has been made both more liberal and more complex in recent years by the growing recognition among philosophers of science that the lines of empirical anchoring of theoretical concepts, the "correspondence rules" which give observational meaning to non-observational terms, cannot be conceived simply as explicit definitions, operational definitions or reduction sentences. Feigl (11, p. 15) sums up the modern position: "... An all-around satisfactory and fully precise explication is difficult, but ... confirmability-in-principle (for statements) or logical connectibility with the terms of a suitably chosen observation basis (for concepts) is the *explicandum* of at least a necessary condition for factual meaningfulness." And Danto (8) is referring to the newer view in his assertion that "a person who demands that every properly scientific term be redeemed (without remainder) with observational equivalents, merely betrays allegiance to a superannuated empiricism." The paragraphs which follow contain a discussion of two closely related aspects of the modern meaning criterion for theoretical terms: 1) theoretical terms are not defined by or reducible to observation terms, but are related to the latter, directly or indirectly, in only a probabilistic manner; 2) the sentences which mediate this essential though probabilistic anchoring of theoretical terms to the observation base are the empirical postulates of the science as well as formal, arbitrary, creative definitions.

The probabilistic nature of the meaning determinants of theoretical terms is perhaps most clearly presented in Carnap's (5) distinction between true theoretical terms and "disposition terms." As described by Carnap[3], disposition terms are mere substitutes for their corresponding observation terms, fully reducible to observation terms, although not themselves direct responses to objects and events. The behaviorist influence and, more recently, the demand for operational definition of every psychologic concept may be viewed as attempts to restrict the behavioral sciences to observation and disposition terms, a restriction corresponding to the original empiricist criterion. While Carnap (5) and Feigl (13) are careful to point out the salutary effect these efforts have had in a discipline that, not many decades past, was more metaphysical than scientific, both are agreed that in practice in psychology, as in other sciences, most of the useful non-observational terms are probabilistically anchored. Carnap (5) gives the following example: Interpretation

[3] Apparently there are differences in the meaning given to "disposition term." For Feigl it is the employment of a term in a postulate system, rather than its probabilistic anchoring, which gives it theoretical rather than disposition status. He writes (12): "Moreover, dispositional concepts frequently and significantly have a statistical rather than a fully determinate form. A typical example from medicine would be the concept of *dementia paralytica* as conceived *before* the etiology in terms of the micro-organism was known. In this case, as in the case of many other diseases defined in terms of correlation clusters of symptoms, the observable symptoms serve only as probabilistic indicators of the 'disease entity.'" Once the relationship between the symptom cluster and a micro-organism is postulated, however, *dementia paralytica* becomes, for Feigl, a theoretical term. We follow Carnap's usage, in which any term the meaning determinants of which are probabilistic is a theoretical term. For Carnap, *dementia paralytica* would be a theoretical concept from the outset.

of a concept such as "I.Q. over 130" as a disposition term identifies it with its defining empirical objects and events, certain test results, while interpretation of the concept as a theoretical term provides that no observation (or test) or combination of observations is regarded as absolutely conclusive evidence for the "presence" of the concept, but is accepted as only probabilistic evidence. Thus, in the example, if the concept is viewed as a disposition term and a subject scores below 130, it *must* be concluded that he does not have "an I.Q. higher than 130," even though it be known that he was very depressed at the time of testing. A science may of course employ both disposition and theoretical terms in its non-observation language, and it may be that many psychologists would consider "I.Q. over 130" as fully defined by test results. If "superior intelligence" were substituted in the example, however, there could hardly be disagreement with Carnap's conclusion that this concept, as generally used, cannot be considered to be fully defined by test results or any other finite set of observations.

The second aspect of the modern meaning criterion for theoretical terms emphasizes the dual nature of meaning determinants: empirical postulates as well as formal (tautologic) definitions are important. Carnap (5, p. 48) makes clear his opinion that a discussion of the meaning of theoretical terms cannot omit the empirical postulates of the theoretical system in which the term occurs. Reference has been made earlier to the concept of "empirical analysis," which focuses upon the non-arbitrary element of the meaning of scientific terms. Clearly, "general paresis" does not mean today what it did prior to the empirical confirmation of the postulated relationships between a certain cluster of behavioral symptoms and various evidences of luetic infection.

As a science develops an increasing body of more highly confirmed postulates, the importance of the empirical element in the determination of meanings of its terms becomes gradually greater as compared to the creative, arbitrary elements[4]. There are always, however, "leading edges" of any science where the creative element remains predominant. In the earlier stages of a science, or at these leading edges, the confirmation of postulates is very tentative and largely impressionistic, and control of observational tests by experimental or statistical methods is infrequent.[5] As long as the crea-

[4] There have been some criticism and some misunderstanding of the tautology, the purely formal, empirically empty statements ("Bachelors are unmarried adult males") which contain no new information about the world of observables but merely prescribe how terms are to be employed. Definitions are tautologies, whereas sentences postulating relationships between concepts with independent empirical meaning are not. Kardiner, Karush and Ovesey (23) criticize some of the principal concepts of Freudian theory on the ground that their anchoring sentences are tautologies. We have argued that this is true of most newer concepts and does not, itself, make for weak theory. There is no methodologic error in defining "libidinal cathexis" in terms of "behavior indicating interest in." The energic concept "libidinal cathexis," thus tautologically defined, might suggest some testable relationships with other independently defined concepts, relationships which would not be suggested by the original "behavior indicating interest in" concept. It is perhaps easier for some to conceptualize a relationship between variations in mother-child experiences and adult energy cathexis to mother figures. If so, the tautology has served a potentially fruitful purpose and no harm has been done. The danger of the tautology is that it may be mistaken for a postulate. In the example, the assertion that "'behavior indicating interest in' may be roughly translated 'libidinal cathexis'" may be incorrectly rendered "'behavior indicating interest in' *is correlated with* (or *is caused by*) 'libidinal cathexis'," with the implication that these are testable statements containing empirical information. Tautological statements are, of course, untestable because the two terms have no independent meaning. Like the statement with an inconsistently anchored term, they are un-disconfirmable, but even more certainly so—by definition, proof against disproof. Mistaking such statements for postulates leads to wasted efforts at empirical testing, the false impression of added knowledge and complacent satisfaction with a theoretical system which may be relatively devoid of empirical content and weak in explanatory and predictive power.

[5] Theoretical systems in which the higher order concepts are derived by factor analysis may represent an exception. In such systems the creative element is important chiefly in the initial selection of observation categories and in the naming of factors.

tive element of the meaning determinants remains important, avoidance of the errors of looseness must continue to be a major concern of the conceptualizer. Psychiatry and the psychology of human behavior are just such primitive sciences or leading edges. The modified criterion of meaning in no way weakens the argument that in the earlier stages of a science, to get the spiral moving, careful attention to avoid the hazards of loose conceptualization is of the greatest importance. This we take to be the central thesis of Reid and Finesinger (37), who write: "Exact knowledge of the truth of any statement presupposes a correspondingly precise understanding of the semantical references expressed by the statement. Such a precise understanding of verbal meanings can, in a field like psychiatry, only rarely be inferred from the context, and therefore it must usually be furnished by clear and unambiguous definitions. . ." We agree with Reid's (35) opinion that, "generally speaking, psychiatrists need more, rather than less, faith in *good* definitions."

It is sometimes argued that early and careful definition of terms impedes research by stifling the creative and flexible use of language, and that careful conceptualization should be postponed for many turns of the spiral. Richfield (38) effectively points out that such an argument confuses vagueness with generality. A precisely defined term will avoid vagueness, and its boundaries will be as clear as possible, but it may include as much or as little as is desired. It may also be flexibly modified in keeping with new empirical findings without sacrifice of clarity. Our view is that even the early turns of the spiral are impeded by lack of concern for logical consistency and reliability of terms.

Within the epistemologic framework of the modern meaning criterion, we shall attempt to describe in greater detail the attributes of a "good" definition. To avoid the conceptual error of unreliability, a theoretical term must be anchored by (a) observation terms (however probabilistically and however indirectly); (b) a set of sentences without logical inconsistencies; (c) a set of sentences explicitly stated and therefore publicly available.

A term for which observation has no implication, a term like "soul" or "spiritual," when isolated by definition from empirical referents, has no meaning in empirical science.

A term whose anchoring sentences are logically inconsistent, a term so loosely conceived that a given observation may on separate occasions count both for and against the "presence" of the concept, renders impossible the empirical disconfirmation of the postulates in which it occurs. Such a term is an invitation to those *ad hoc* manipulations of theoretical terms so often employed to protect cherished postulates (more likely called "laws" or "discoveries") from observational test by rendering them proof against disproof. This is probably the most frequently encountered source of loose theory in the behavioral sciences. It is certainly the source most frequently cited by analytic philosophers when they are confronted with psychiatric theory (8, pp. 317–318; 11, pp. 14–15; 14; 21, 33, pp. 39–42; pp. 214–216). "Hallucinations" may be only probabilistically related to "schizophrenia" (if the latter is interpreted and used as a true theoretical term), and is not therefore a necessary condition for the diagnosis. This probabilistic relationship does not alter the requirement that the implication of "hallucinations" for or against the diagnosis must be clearly and unambiguously specified. "Hallucinations" cannot at one time count for and at another against the "presence" of schizophrenia. It will be apparent that, under the circumstance in which the correspondence rules of "schizophrenia" are so loosely and inconsistently conceived that any combination of behavior might be so classified, the term "schizophrenia" becomes meaningless in that no postulate about its relationships (whether with genetic, biochemical or childhood experiential vari-

ables) could ever be disconfirmed. Actually "hallucinations," as an observation term, represents the anchor point of one of the more consistent lines of anchoring of "schizophrenia." There is reasonable agreement among psychiatrists that "hallucinations" weighs mildly in favor of the diagnosis, whereas "no hallucinations" weighs mildly against. Thus our example exaggerates the difficulties of psychiatric theory; the term "schizophrenia," like most terms currently used in psychiatry, is not totally without meaning but has some logically consistent lines of empirical anchoring. But a major source of the problems encountered in research on schizophrenia will be appreciated if consideration is given to the inconsistency of including in the diagnostic criteria at one time "flattening of affect" alone, and at another shifting without specification to include all affect of abnormal amplitude, both high and low.

The anchoring sentences of theoretical terms must be publicly and explicitly stated. This demand derives from the basic scientific requirement of objectivity: intersubjective agreement. It ensures that some effort will be made to relate the theoretical term to the public world of objects and events, and increases the likelihood that the theorist will become aware of logical inconsistencies in the various anchoring sentences. The attempt to give explicit, public statement of the meaning of a theoretical term thus serves as a useful reminder to the theorist of the requirements of science and as a first index of his success in meeting these requirements for empirical significance or meaningfulness. Hook's (21, pp. 215–216) request that psychoanalytic theorists provide public clarification of the concept "oedipal" is a legitimate request and not an unwarranted restriction upon creativity, for he makes it clear that he recognizes the abstract and complex nature of theoretical terms as specified in the modified meaning criterion. The test of the meaningfulness of a theoretical term is its reliable use in various specific observational and verbal situations by those trained in the arbitrary definitions of the theory and familiar with the state of confirmation of its empirical postulates.

The observation terms, the other theoretical terms, the definitions or postulates, and the probabilistic relationships which give meaning to a particular theoretical term must at first be employed consciously in the application of that term. These meaning determinants may "wither away," in the words of Mandler and Kessen (30, p. 85), so that the theoretical term may, for a time, seem to become an observation term, reliable in response to objects and events. When used by a particular group of investigators, reliability may be achieved without conscious attention to the lines of anchoring. It will, however, be necessary for the investigator to bring these lines back into conscious focus at regular intervals for purposes of revision, teaching, to report research results and to restore reliability when it wanes.

Thus, for a group of persons working in close association, "schizophrenia" or "oedipal" may come to acquire for a time enough implicitly shared meaning to permit reliable usage. Nevertheless, for the reasons given above, the need to make these shared-meaning determinants as explicit as possible remains a constant obligation.

Earlier in this section on the epistemologic bases of errors of conceptualization, attention was called to the contrasting demands of the phase of conceptualization for free creativeness and rigorous attention to empirical anchoring. In the conclusion of the section it may be pointed out that not infrequently the reaction to such a conflict-producing situation is an unsuccessful attempt at compromise, in which rigidity and reification prevent creativity while blocking any rigorous appraisal of the concepts already rigidly adopted. The attitude of those who have effected this fatal compromise seems to be that there is clearly no

need for logical analysis or reliability testing. The terms adopted, whether theoretical or observational—"schizophrenia," "libido," "ego" or "oedipal," "incoherence of thought," "flattening of affect," "anxiety" or "depression"—are such obvious *things* as to be readily recognized by anyone.

SUMMARY

As conceived by many modern philosophers, science is dependent for its advancement upon the combination of observation and rational thought. The interdependence of these two elements may be appreciated if the scientific process is viewed as a spiral in which controlled observation alternates with creative conceptualization. Controls are necessary in the phase of observation to minimize error due to faulty sampling of the material to be observed, observer bias, and unrecognized influence of the observer and field upon the subject.

This paper is focused upon the need for "controls" in the phase of conceptualization. A source of error in this phase is reification of and inflexibility in the use of terms; lack of creativeness and reluctance to modify or abandon concepts in order to incorporate new empirical findings and facilitate future observation; the need to defend, on grounds other than empirical, cherished concepts and theories against all others. Another and equally important error source is centered in excessive looseness in the use of terms, and disregard of the scientific requirement of objectivity, *i.e.*, disregard of the criterion of intersubjective reliability in the use of both observational and non-observational terms. All scientific terms must permit reliable use. Neither the non-observational character nor the postulate-mediated, probabilistic empirical anchoring of theoretical terms justifies neglect of the requirement of intersubjective agreement.

The above principles are discussed against the background of the philosophy of science. In addition, the present paper argues the particular need for conceptual "controls" which results from the relative immaturity of psychiatry as a science. In a second paper, some of the proposed solutions to the conceptual problems of psychiatry will be reviewed and special emphasis given to the testing and improvement of reliability as an essential and often neglected step in psychiatric research.

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A CASE OF MULTIPLE PERSONALITY ILLUSTRATING THE TRANSITION FROM ROLE-PLAYING

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INTRODUCTION

Less than two hundred cases of alternating personality have been published in the literature of psychiatry and psychology. The first well studied published case is that of Mary Reynolds (10), and one of the most recent, that of Eve (12). Despite their rarity, cases of alternating personality have contributed considerable information about psychological processes and psychopathology. This reason alone would justify the reporting of another case, but the present one seems additionally significant because it shows transitions between an imaginary playmate, simple role-playing, and dual personality, with amnesia on the part of the primary personality for the periods of dominance of the secondary personality.

FIRST APPEARANCE OF THE SECONDARY PERSONALITY TO THE THERAPIST

The patient was a twenty-three year old housewife, whose primary personality was known as Betty. She was first briefly admitted and treated at another hospital for convulsions which proved to be hysterical. She was then transferred to the University of Virginia Hospital for further evaluation and treatment. At that point she was somewhat depressed and considered potentially suicidal. In the hospital, the patient recovered rather quickly from the hysterical symptoms and severe degree of depression and was then discharged and treated as an outpatient with further psychotherapeutic interviews. During these interviews, the therapist explored further the patient's early life, which will be described below. The patient stated that as a child she had created an imaginary playmate she called Elizabeth. During one interview, about two months after the patient's discharge from the hospital, she was describing her imaginary playmate to the therapist when she suddenly seemed to become that personality. She sat bolt upright in her chair, at the same time assuming a more relaxed and friendly demeanor, and said to the therapist: "I think it's about time I started telling you about me." The astonished therapist said: "What do you mean?", and the patient replied: "About me, not about her." She then proceeded to describe herself (Elizabeth) and her career more fully.

From this time on, until the disappearance of Elizabeth four months later, it was possible for several observers to study both Betty and Elizabeth under a number of different circumstances. Psychological tests of the two personalities supplemented the clinical observations.

CHARACTERISTICS OF THE TWO PERSONALITIES

Behavior: Betty showed restraint and primness at all times. She was polite and outwardly cooperative. She rarely smiled and gave an appearance of depression. She was exceedingly tense and her hands frequently trembled. She spoke in a low, even and well controlled voice. She sat stiffly and addressed the therapist formally. She ate daintily and sparingly, and was extremely orderly and careful about her dress and belongings. She stayed by herself much of the time and socialized little. She complained of headaches, abdominal cramps and dysmenorrhea; she fainted frequently.

Elizabeth, on the other hand, had a much more outgoing and friendly personality. She relaxed more in her chair and talked

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informally. Her voice was somewhat higher and was characterized by much more inflection and variety of pitch. She often used slang and humor. She was much more gregarious than Betty. She socialized comfortably and made friends quite easily. She was healthy and had no physical complaints.

At the time of our initial observations and for some months thereafter, Betty had amnesia for Elizabeth's periods of dominance. Elizabeth, however, knew all that happened during control by Betty. Elizabeth adopted a rather scornful, patronizing air toward Betty, but was also sympathetic toward her difficulties, especially those of her (Betty's) marriage. She completely denied that she herself was married.

Information gathered from the patient's mother (and other members of her family) revealed that for some time the patient's behavior with her family had shown puzzling, abrupt, alternating contrasts of behavior. Ordinarily, her mother said, the patient was quiet and reserved to the point of being withdrawn. She habitually dressed in the most conservative style. But at times the patient would suddenly brighten, talk animatedly, dress more gaily, and accompany her mother to a local movie theater.

As observed by ourselves and the patient's relatives, the two personalities showed consistent patterns of behavior during the periods of observation.

The transition from one personality to another: The first transition observed by the therapist (or any other psychiatrist) occurred as the patient was describing the imaginary playmate which had become the secondary personality. Subsequent change-overs occurred when the therapist requested one or the other personality to appear. They also occurred during periods of stress. Elizabeth would take control during periods of stress accompanied by extreme tension for Betty. Instead of getting Betty into trouble (as some secondary personalities have done for their primaries), Elizabeth often saved Betty from difficult situations. For example, once Betty took an overdose of sedative pills suicidally, whereupon Elizabeth took control and vomited the pills.

The transitions from one personality to the other observed by us occurred quite quickly. Upon one personality being called during the dominance of the other, the patient would lapse into a state of seeming detachment for about ten seconds and the other personality would then appear. This personality would usually persist until dismissed. We observed altogether about fifty transitions between one personality and another.

Psychological tests: The patient was given a number of psychological tests approximately one month after she had revealed to the therapist the existence of the secondary personality. The secondary personality, Elizabeth, was tested a week prior to Betty. Each personality was examined with seven psychological tests, but because of limitations of space and to avoid the repetition found in many of the results, only important features from the Rorschach Test, Hutt Sentence Completion Test, Orzeck Self Scale (9) and Word Association Test will be presented here. Another article will report fully the data of all the psychological tests.

The Rorschach Test showed that Betty had a general personality structure strongly suggestive of hysteria. Her responses indicated emotional lability and a tendency to mood swings. But they also showed much conscious control of emotions and a strong emphasis upon adapting through conformity to social conventions. Betty utilized a higher degree of fantasy and imagery than is usually seen in the hysteric, an observation harmonious with her ability to create the entirely different personality of Elizabeth.

Elizabeth's personality seemed to have less depth than Betty's. Her responses reflected a dearth of fantasy and imagery and suggested that she strove to be conventional in thought and ideation. She gave evidence of being even more emotionally labile than Betty and with less control.

Social amenities and a need to show good manners influenced her much less than Betty. Lack of social conscience and constraint suggested egocentricity and an inability to delay immediate gratification of impulses for future gain.

Despite the contrast between Betty's highly imaginative hysterical traits and Elizabeth's shallow and more constricted character, the two personalities did show similarities. In their thought processes, both attempted to be correct and accurate in a compulsive manner, but both lacked the capacity to be adequately objective in their perceptions of their environment. Their reality testing was not grossly impaired, but their perceptions seemed to be easily influenced by their emotions.

On the Sentence Completion Test, Betty presented a naive and childish set of attitudes and needs. She showed overly strong needs to be good, passive and compliant. She appeared to think of herself as a little girl still under her grandmother's domination (to be described below). She also seemed to have adopted some of her grandmother's rigidity, for she displayed an inflexible, internalized set of rules and a strict conscience.

Most of Elizabeth's sentence completions reflected attitudes and needs opposite to those of Betty. Betty's completions reflected passivity and dependency, while Elizabeth's exhibited a need for rebellion and independence. Betty indicated that she valued being good, but Elizabeth felt being good was a bore. Betty's attitude toward marriage was negative, while Elizabeth's was definitely positive. Betty derogated males but Elizabeth complimented them. Betty feared many things, while Elizabeth stated, somewhat defensively, that she feared nothing.

Betty's associations to the Word Association Test predominantly revolved around phobias and affects of fear and disgust. She exhibited major conflicts in sexual and social areas, with most of her ideation in these areas reflecting negative feelings. Elizabeth's associations were frequently diametrically opposite to Betty's. In contrast to Betty's frequent associations of "filthy," "dirty," "frightening" and "sickening," Elizabeth gave more hedonistic associations such as "pleasure" and "happiness." If not indicating pleasure, her associations at least tended to be neutral. Many similarities of associations occurred, particularly for the more neutral stimulus words. A majority of the identical responses were popular ones. When away from sensitive topics such as sex, Betty and Elizabeth, for the most part, showed similar associations.

On the Orzeck Self Scale, which is more open to conscious control and evaluation than any of the foregoing psychological tests, Betty gave 47 adjusted answers and 43 maladjusted answers, and Elizabeth gave 89 adjusted and only four maladjusted answers. A chi-square test of significance of difference yielded a probability value of $<.001$. It would seem that Elizabeth was significantly better adjusted than Betty, at least insofar as conscious concept of self is concerned.

The results of the psychological tests suggested that Elizabeth's personality counterbalanced Betty's. Elizabeth's traits, attitudes and behavioral patterns seemed designed to offset or to control those of Betty's that were maladaptive. She maintained a freedom of expression and enjoyment unavailable to Betty. The attempted compensation sometimes went too far in the opposite direction, toward important loss of control. And in spite of their differences, the personalities seemed to be similar in the areas not charged with anxiety.

EARLIER HISTORY OF THE PATIENT AND OF THE ORIGIN AND CAREER OF THE SECONDARY PERSONALITY

The following information was obtained chiefly from the patient as both Betty and Elizabeth. Members of the patient's family corroborated the main facts.

The patient was an only child. She was reared in the home of her paternal grandparents, with whom her parents lived during their marriage. The patient's father committed suicide when he was 24 and she four years old. The patient's mother was an easy-going, but passive and rather ineffective woman who worked out of the home after her husband's death. She entirely abdicated the direction of the patient's early life to her mother-in-law, the patient's grandmother. The latter was a formidable tyrant, of stern, unyielding morality based on strict adherence to Victorian standards. She completely outlawed sex as a legitimate pleasure and most other pleasures as well. The training administered by the patient's grandmother seems to have contributed extensively to the development in her personality of a very marked and indeed disabling shyness, especially with regard to members of the opposite sex. A neighbor added to her fears of sexuality by assaulting her sexually when she was five. As she reached adolescence, the patient attempted to suppress and conceal the signs of her developing womanhood by wearing baggy dresses and avoiding all make-up or other adornment. In her school years, she seems to have been a lonely child and her contacts with other children became reduced by her grandmother's snobbish disparagement of them as inferior to the patient. Despite the tremendous handicaps placed upon her social life, the patient managed to get along fairly well in school and eventually graduated from high school.

When the patient was about seven years old, she began to substitute for the satisfactions of real playmates the lesser ones of an imaginary playmate whom she called Elizabeth. Betty spent many hours in play with the fantasied playmate. Elizabeth was always allowed to do the things that she herself was forbidden to do, and always had the courage to undertake what the patient herself feared to do. The patient maintained this playmate well into high school. In grammar school, Elizabeth was the subject of fantasy adventures that the patient felt she dared not undertake herself. Throughout the school years the patient came to identify herself more and more with Elizabeth and to try out in life various little adventures while pretending that she was Elizabeth. For instance, as Elizabeth she would go to the local candy store to buy and eat some of the candy which was denied her as Betty. Social activities such as parties were usually forbidden her and on the few occasions when she was allowed to go she felt quite frightened at the prospect. So she extended the habit of pretending that she was Elizabeth and went to these few parties as Elizabeth. As Elizabeth she could be more free and enjoy herself. This pattern became more and more firmly fixed in her senior year of high school. It seemed the only way for her to enter into any stressful situation outside of her own home.

At the time of her graduation from high school the patient had had very few dates, and only one boy had shown more than a passing interest in her. The patient took a position as a clerk-typist in a small business. At this time she fell even more under the dominance of her grandmother who now censored her mail and her checkbook and supervised the most minute details of her daily life. When her only suitor finally proposed to her, she seems to have accepted him as much to escape from her grandmother as because of any attraction for him. They were married when the patient was nineteen. Almost immediately she discovered that her husband was a brutal ruffian and also a sexual deviant. The patient fled from the honeymoon but her grandmother forced her to return to her husband. The patient then managed to stay with her husband for three years, probably only because his frequent absences from home gave her respite from his cruelty. On one occasion when her husband behaved in a particularly cruel manner, the patient suddenly became unusually aggressive and chased him from the home. She (*i.e.*, Betty) had amnesia for this episode

(subsequently pieced together from the accounts of Elizabeth and relatives) in which she had asserted herself in the personality of Elizabeth. Elizabeth then remained in control for some six months. During this period she separated from her husband, had a brief love affair with another man, and finally returned to live with her grandmother. She dressed in a more feminine style and showed the relaxed, friendly behavior characteristic of Elizabeth. After a time "things became dull again," and control lapsed to Betty. During the next few months, Elizabeth took control for brief periods of innocent play, but was never in control for long.

The patient's husband sued for divorce, charging the patient with adultery. The patient allowed the divorce to proceed without defending herself, although urged to do so by her lawyer. The stresses of renewed living under her grandmother's control (which Betty could not shake off) and of the divorce seem to have been precipitating factors in the occurrence of the hysterical convulsions which led to the patient's admission to the hospital previously mentioned.

PSYCHOTHERAPY AND SUBSEQUENT COURSE OF THE DISORDER

After the emergence of Elizabeth during a therapeutic interview, additional interviews with members of the family confirmed the existence of the alternating personality for some years back. The members of the patient's family had noticed the sudden shifts of her personality and attributed them to her being "peculiar," without further understanding them. Elizabeth also supplemented the history since she had a full memory for the periods of amnesia in Betty's life.

Shortly after the identification of Elizabeth as a secondary personality, the patient's divorce proceedings became more embroiled. A leakage of news in her community about the patient's affair with another man provoked a painful scandal which only subsided slowly. During this stressful period Elizabeth seemed to gain control more often and for longer periods. Betty, now having some awareness of the activities of Elizabeth (although no memory of them), became more depressed and made the suicidal attempt already mentioned.

The therapist gained the alliance of Elizabeth for the treatment of Betty. Whenever possible, the therapist talked with Betty. The therapist attempted to deal first with the patient's fears and guilt regarding sexuality, and second, with her lack of assertiveness. Success accompanied these efforts and Betty gradually became more relaxed, less anxious and more assertive. During this period, Betty gradually recovered the memories of much of the periods during the dominance of Elizabeth. There then ensued a period during which, although the personalities continued to alternate, each had full awareness of the other. (This resembled somewhat the earlier period of conscious role playing.) After four months of therapy, Elizabeth dropped out altogether and Betty remained dominant. Although still susceptible to tension, depression, and physical symptoms related to these, Betty has remained dominant ever since (now two years) and her condition has gradually improved. She finally became independent enough to leave her grandmother's home and move to another town, where she is gainfully employed. Betty now recalls fully everything she did as Elizabeth and has insight into the value of the secondary personality in protecting her from severe stresses.

DISCUSSION

The two types of multiple personality: When a dissociation producing dual or multiple personalities occurs, the secondary personalities may belong to one of two types. In some instances, the secondary personality differs little from the primary one except in lacking the memories of the first personality. The secondary personality has to learn anew

many skills quite familiar to the primary but forgotten personality. It does not recognize the friends and family of the primary personality. But in fundamental attitudes the secondary personality does not differ significantly from the primary one. Poulting-Poultney, described by Franz (4), and the case described by Maddison (8), provide examples of this type of case. It seems to occur especially after severe physical stress, *e.g.*, an explosion or other cause of brain damage. In these types a two-way amnesia between the personalities occurs.

In the second type of dual personality, the secondary personality has attitudes which differ significantly from those of the primary personality. Such secondary personalities can express prohibited behavior not permitted to the primary personality. Everyone perhaps has fantasied identifications of himself with other persons he envies or admires. Under appropriate conditions, *e.g.*, hypnosis, these identifications may appear as definite roles. Harriman (5) found that when he put subjects into hypnotic trances and with suggestions erased, so to speak, their primary personalities, *ad hoc* secondary personalities which he had not in any way suggested filled the vacuums of personality and were manifested plausibly and consistently. For the construction of these ostensibly new personalities hypnosis provided the mortar and preceding fantasies the bricks. The materials for such secondary personalities lie within everyone, but are better organized in some persons than in others. In their extensive review of multiple personality, Taylor and Martin (11) mention the hardening of an habitual role into a secondary personality. The patient described by Davis (2) first wrote a novel in which the hero lived the author's wishes. In a later period of stress, the novelist had a fugue, and became the personality of his novel's hero.

In the second type of dual personality, the primary personality is often amnesic for the experiences of the secondary personality; the secondary personality, however, knows and remembers both its own experiences and those of the primary personality. Taylor and Martin (11) have reviewed the various relationships which can occur between the different personalities.

The present patient is clearly an example of the second type of multiple personality, for her secondary personality filled a behavioral gap, not merely a mental one. The next consideration is how such a secondary personality develops and how, so to speak, it gets out of the bottle.

Are multiple personalities artifacts of psychotherapy? Critics of reports of multiple personality have sometimes asserted that they result from the behavior of the therapist, who differentially reinforces certain kinds of behavior on different occasions and thus gradually evokes the fully developed secondary personality. It is certainly possible to create multiple personalities during hypnosis, and the different personalities may have amnesia for each other just as do the naturally occurring cases (5, 6). Therapists using hypnosis have also stumbled upon previously unrecognized dual or multiple personalities (1, 3). Even without hypnosis, the special interests and expectations of a therapist can perhaps enhance the differences of dual personalities, and conceivably even create a dissociation not previously existing without intending to do so. We cannot, however, attribute all instances of multiple personality to the (conscious or unconscious) reinforcement of one aspect of the personality by a therapist. The literature contains numerous instances in which clear evidence was obtained from persons other than the patient of the manifestations by him of two or more personalities before he first encountered a therapist (4, 7, 8). The present case provides another example of this.

Are multiple personalities fashioned by parents? We should consider also the possibility that the relatives of a child when young may differentially reinforce special aspects

of the child's behavior and thus make a crevasse in the personality. For several reasons, this seems unlikely as a complete explanation of such cases. In the first place, the onset of the dual personality seems often to be sudden rather than gradual. In the present case, the "personality" was built gradually, but was finally assumed (with amnesia) suddenly. A second point of possible relevance in the present case is that Betty created and used the secondary role secretly for a considerable time. She concealed her imaginary playmate and its use from others for many years. They only became aware of it during the later episodes when the secondary personality attained control for longer periods. Third, the secondary personality expresses more freely impulses and attitudes which the surrounding dominant relatives find objectionable. Parents do sometimes, however, unconsciously reinforce behavior of which they consciously disapprove, and we cannot entirely exclude their influence in promoting a secondary personality. In the present case, there is evidence (unfortunately, from the patient's reports only) that the patient's grandmother emphasized sexuality greatly for the patient. Her words dwelt on the repulsiveness of sex, but smiles on her face as she talked betrayed the fascination and pleasure of the subject for her. Thus the patient's grandmother may have reinforced both the inhibitions that crippled Betty and the craving for their expression that only Elizabeth achieved.

The patient's grandmother may have promoted but did not create, the secondary personality. What did? Presumably, the severe inhibitions imposed on the child led to the repression of many normal impulses. The child's isolation encouraged fantasies that gradually became organized into the personality of an imaginary playmate who enjoyed the freedom of which the patient was deprived. The transition to conscious role-playing in adolescence seems a natural development, but the final occurrence of a split with amnesia less so.

The significance of amnesia: Amnesia is the principal and perhaps only psychological feature which distinguishes instances of dual or multiple personality from other alternations of behavior. At different times of their lives, not necessarily far separated in time, many persons show variations in behavior as great as or perhaps greater than those occurring between Betty and Elizabeth. The clinical observations and the psychological tests of this case all showed that the personalities of Betty and Elizabeth differed, but also that they overlapped and were far from being entirely discrete or mutually exclusive in all parameters studied. Indeed, in the areas of living which did not make the patient anxious, as sex did, the personalities did not differ greatly from each other. We believe, therefore, that the amnesia of the primary for the secondary personality distinguishes the two personalities as much as the details of the differences in the personalities themselves.

Since we have emphasized the importance of the amnesia in patients of this kind, we should consider by what criteria we establish the occurrence of an amnesia. How can we be sure the patient was not an actress? We have no final answer to this question. We can, however, draw attention to the consistent behavior of the secondary personality over a lengthy period of observation by ourselves and others. If the patient had achieved this degree of consistency by conscious acting only, then she was not a mediocre, but a highly skillful actress. Moreover, the evident effort required by the patient to recall, as she finally did, episodes in the career of Elizabeth which she had previously been unable to remember, contributed to an impression of sincerity and genuine forgetfulness.

We shall next consider the question of the adaptive purpose of the amnesia. During the second phase of the existence, if we may call it such, of this personality, the primary

personality (Betty) was perfectly conscious of her assumption of the role of Elizabeth and remembered what she did when in that role. Subsequently, she ceased to be able to remember. Since the amnesic phase of the disorder occurred during the severe stress of the patient's marriage and brutal treatment by her husband, we may suppose that this stress somehow increased the division in the personality which then became somewhat consolidated. We may also infer that Betty's strongly developed sense of duty made it more difficult for her to escape from the marriage consciously as she had escaped from less unpleasant situations when younger. It was as if the escape provided by the personality of Elizabeth had been developed for such occasions, but on this one could only be used without Betty's awareness. In harmony with this view is the fact that, as Betty became more independent, the patient again passed through a phase of alternating personalities with full memory for the periods of dominance of each.

SUMMARY

A case of dual personality occurred in a young woman who had developed an imaginary and less inhibited playmate as a child. During her school years she consciously assumed the role of this playmate and adopted it when she wanted to enjoy otherwise prohibited or inhibited behavior. At the age of twenty-two she began assuming this personality, with amnesia for its periods of dominance. The secondary personality retained awareness of the activities of the primary personality. The patient became increasingly tense and depressed and required treatment.

Treatment directed at reducing the patient's fears of sexuality and of being assertive led to a gradual cessation of the appearances of the secondary personality. During the period of treatment a phase occurred when the personalities alternated, with each personality retaining the memory for the periods of dominance of the other one.

The two personalities were significantly different as judged by clinical observations and psychological tests. Nevertheless, the two personalities also had in common many aspects of attitude and behavior in terms of the original personality not affected by the sources of the patient's anxiety.

The occurrence of the dual personalities antedated by two years the patient's first contact with the therapist. The phase of conscious role-playing antedated this contact by many years.

An unusually harsh and restrictive upbringing seems to have made at first difficult and later impossible the expression of sexual and aggressive impulses except in the role of the imaginary playmate and later in the behavior of the secondary personality.

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MUSCLE TONE, SPASTICITY, RIGIDITY

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The terms *muscle tone, spasticity* and *rigidity* have long had their place in the medical vocabulary; but however well they may be established as clinical concepts, our knowledge of their physiologic and pathophysiologic basis has remained deficient. Nevertheless, considerable progress has been made in the last 25 years. This review is designed to summarize some of the pertinent data without attempting to defend the opinions expressed by the various investigators.

MUSCLE TONE

In this discussion, *muscle tone* refers solely to what is known as "reflex tone," for the so-called cellular tone (3)—important though it may be in smooth muscle—appears to have little physiologic significance in skeletal muscle. Muscle tone has been defined in clinical terms as the "slight constant tension characteristic of healthy muscle owing to which the limbs, when handled or moved passively, offer a definite resistance" (34).

Dual Innervation: The knowledge that tonus arises on a reflex basis dates back to 1860, when Brondgeest (6) demonstrated in the frog that section of segmental dorsal roots resulted in loss of posture of the hind limb. Further investigation revealed that depriving the muscles of their skin—thus severing the cutaneous nerve fibers—had no such effect, indicating that this reflex tone was maintained by proprioceptive impulses from the muscle itself (54).

It was suggested that two types of muscle fibers must exist, one for sustained tonic and the other for quick phasic contraction. The differences in speed and duration of contraction resulted in a division of muscles into slow and fast groups, and an attempt was made to correlate the speed of contraction with the color of the muscle, which indicates its content of myoglobin.

Denny-Brown (9) observed that slow, red muscles are those in which the stretch reflex is most easily elicited and is best maintained as a static reaction, whereas pale, rapid muscles fail to maintain posture or to exhibit static reflexes. Anatomic evidence for the presence of well defined tonic and phasic muscles in the cold-blooded animal was presented by various authors (28, 39, 65). Pertinent electrophysiologic observations were made by Göpfert and Schaefer (19), who recorded electric potentials confined to the muscle end-plate which they called "end-plate current." Tasaki *et al.* (69, 70) in 1944, and Kuffler and Gerard (40) as well as Kuffler, Laporte and Ransmeier (42) in 1947, showed these local potentials to arise from small myelinated nerve fibers whose excitation resulted in slow, non-propagated muscle shortening. The combined action of these small nerve fibers was capable of producing in the muscle a mechanical tension of 10 to 15 per cent of the maximal contraction.

However, this concept of dual innervation of muscle for direct tonic contraction was generally denied in respect to mammalian muscle until recently, when Granit and co-workers (23) reported the existence in the cat of two types of motoneurons. The smaller of these, of tonic type, exhibited prolonged lower-voltage and lower-frequency activity subserving slow sustained contraction and maintenance of posture. The larger, of phasic type, showed brief higher-voltage discharges subserving quick contraction.

Subsequently Eccles, Eccles and Lundberg (10), by means of intracellular record-

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ings, established certain criteria for motoneurons innervating fast and slow muscles. Studying the gastrocnemius-soleus, they found that motoneurons supplying slow, red soleus muscle exhibited a longer post-spike hyperpolarization and a slower conduction velocity in their motor axons than did motoneurons supplying the fast, pale gastrocnemius. Buller, Eccles and Eccles (8) concluded that differentiation of muscles into fast and slow types was really a property of their motoneurons. They observed that in the two-week-old kitten all skeletal muscles were slow. Only after several weeks did the muscles differentiate into fast-pale and slow-red types. They sectioned the nerves supplying muscles which would become fast and slow types and cross-united them. Five months later the tissues of the soleus, which normally developed into a slow muscle, had been converted into a fast one.

Despite these important contributions, our knowledge concerning the relationship of motoneurons to red and pale, tonic and phasic, or various other types of muscle is still incomplete indeed.

Gamma System: Much firmer evidence supports the concept of another type of motor system for maintenance of mammalian muscle tone, this without direct tonic contraction. In 1935 (56) it was found that stimulation of small-diameter ventral-root fibers produced no shortening of the muscle beyond that caused by stimulation of large motor fibers. A few years earlier (13), these small fibers had been shown to make up about 30 to 35 per cent of the entire ventral-root fiber outflow. Leksell (45), in studies using the cat, determined the conduction velocity of these fibers to be 20 to 40 meters per second, and called them gamma fibers, in reference to the gamma velocity of their axons in the Erlanger-Gasser (14) classification. By removing the large alpha fibers selectively, he also showed that the small gamma fibers were destined for the muscle spindles. These observations were later confirmed by other investigators (36, 41).

The muscle spindles have now been shown to be important links in the regulation of movement. They are elongated, encapsulated structures containing several modified muscle fibers, called *intrafusal fibers.* As mentioned above, these intrafusal fibers receive their efferent innervation from the small-diameter gamma motoneurons, although very recent studies suggest that large, alpha-type motor neurons may also participate in their innervation (27). The afferent innervation of the muscle spindle is twofold. Wrapped around the middle of the intrafusal fibers are sensory-nerve fiber terminals called primary or annulo-spiral endings, a sensing device for muscle length, the single large myelinated axon of which takes an afferent course, entering the spinal cord via the dorsal root. At the poles of the intrafusal fiber are other, smaller, delicate, finger-like terminations that have been designated *secondary* or *flower-spray* endings, the axon of which likewise enters the cord through the dorsal root. Very little is known about these afferents.

The specialized muscle spindles have been shown to be the sensory receptors for the stretch-reflex. Their long axes lie parallel to the main (extrafusal) muscle fibers and participate in their attachments. Active contraction of the muscle therefore relaxes the spindles, whereas passive traction on the muscle puts the spindles under tension, setting off impulses in the annulo-spiral endings that are carried in an afferent direction to the spinal cord. Here, via a single synapse, they connect with the large alpha-type anterior horn cells, eliciting a reflex discharge in the motoneuron going to the same muscle from which the afferent volley originated (Figure 1). Extension of the muscle thus results in a reflex contraction that tends to resist the extension.

This short-latency, dorsal to ventral-root

reflex was first suggested by Hoffmann (32), then demonstrated by Eccles and Pritchard (12), and later further investigated by Renshaw (59), Lloyd (48) and others. Also, Magladery and his group (50) have made important contributions to our knowledge of the monosynaptic reflex. This reflex, on excitation from the spindles, is elicited in the agonistic and synergistic muscles. In addition, there is simultaneous inhibition in the antagonist through a descending collateral and probably inhibitory interneuron (11).

The small motor gamma efferents that innervate the intrafusal fibers at their contractile poles cause discharges in the spindle afferents. They do this by making the intrafusal fibers contract, thus producing a mechanical stimulus in the sensory spindle endings equivalent to passive stretch of the whole muscle. The muscle shortens reflexly until the increased rate of spindle discharge is offset, which occurs when the muscle has shortened to the same extent as have the spindles. Hence, spindle discharge depends not only upon stretch but also upon innervation from the fusimotor gamma fibers, the latter regulating the sensitivity of the muscle spindle to stretch. Except for the time when the spindles are inhibited by active muscle contraction, they discharge continuously in a tonic fashion, maintaining the ventral horn cells in a state of constant facilitation. The activity of the muscle spindles is of great importance for motor performance. In fact, Granit (22) considered it probable that all types of motor performance except the fastest and most powerful occur with simultaneous gamma adjustment. In most acts of movement the alphas do not start discharging until the gammas have initiated the activity.

It now seems to be generally agreed that an important part of what is called "tone" in striated muscle is this sustained, asynchronous, stretch-evoked reflex activity of the gamma system (49).

Alpha Neuron Activity: The gamma system probably is not the sole factor causing muscle tone, however, since alpha neuron

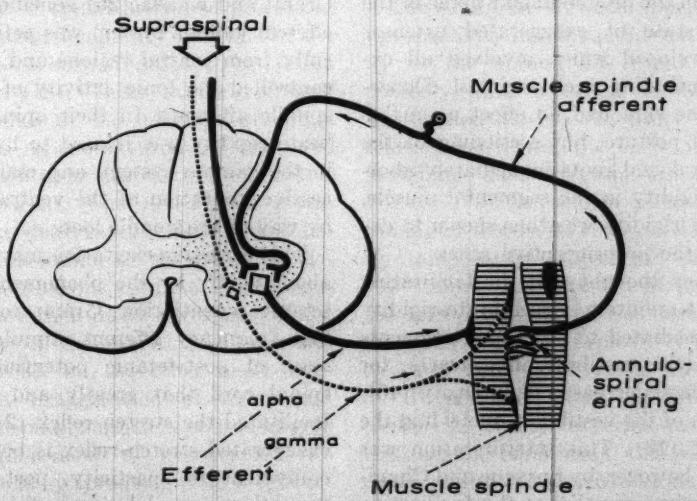

FIG. 1. Diagram of spindle loop. Muscle spindle in parallel with main muscle fiber. Spindle afferent neuron connects in spinal cord by single synapse with alpha-motoneuron which innervates main muscle fiber. Efferent motor innervation of spindle occurs through small-diameter gamma fiber. Diagram depicts dual innervation of muscle: directly through alpha route, and reflexly through gamma route.

activity seems to have some contributing effect. Brookhart (7) has presented evidence for the existence of small, slowly conducting pyramidal fibers that may be responsible for the tonic activity of pyramidal function; and Adrian and Moruzzi (1) found spontaneous, persistent discharges in the "resting" pyramidal tract of the cat corresponding to waves from the motor cortex. Also, Lance (43) has speculated on tonic pyramidal actions. Though these discharges are not great enough to produce overt movements, they may possibly provide a constant subliminal facilitating effect.

ABNORMAL STATES OF MUSCLE TONE

It is not surprising that the gamma loop, like any other neural structure, is under the control of a number of mechanisms. Of these, the supra-spinal pathways in particular seem to be highly important in relation to certain pathologic states of muscle tone, namely, spasticity and rigidity. Sherrington's (63) classic experiments demonstrated that shortly after transection of the brain stem in the intercollicular plane of the animal a state of exaggerated extensor rigidity developed which involved all extremities, including neck and tail. Denervation of the skin had no effect upon this exaggerated posture, but sectioning of the appropriate dorsal roots immediately abolished the rigidity in the segmental muscle. Decerebrate rigidity was thus shown to depend upon the proprioceptive reflex.

It has been thought that the facilitating process that maintains decerebrate rigidity is in part mediated to the spinal segments by way of the vestibulospinal tracts, for section of these decreased its intensity (46). Destruction of the vestibular nuclei had the same effect (16). This interpretation was challenged, however, by Sprague and Chambers (66), who argued that the facilitating effect was transmitted from the fastigial nuclei of the cerebellum instead. The destruction of the vestibular nuclei, they maintained, had simultaneously destroyed the restiform bodies and thereby the fastigiobulbar tracts.

Another important source from which facilitating impulses arise is the brain stem reticular formation, which exerts its influence by way of the facilitating reticulospinal tracts (62) that lie widely scattered throughout the anterolateral white matter of the spinal cord. According to Sprague and Chambers (66), only a small part of this reticular formation is necessary to maintain the decerebrate state, provided this part is activated from other sources.

Investigating the gamma system in cats, Granit and Kaada (26) observed that this system is highly active in the decerebrate state, and that stimulation of the anterior cerebellar lobe and of certain regions in the brain-stem reticular system, which previously had been found to facilitate and inhibit muscle tone and movement (51, 60), also facilitated and inhibited muscle spindle activity. Chlorpromazine, acting upon the reticular formation, likewise had an inhibitory effect upon spindle activity (31). Granit and Kaada (26) concluded that the efferent gamma system was activated tonically from central regions and, in its turn, controlled the tonic activity of the muscle spindle afferents. In their opinion, decerebrate rigidity was related to hyperactivity in the gamma system, and maintained excessive excitation of the ventral horn cells by way of the spindle loop.

This excessive excitation may be brought about partly by the phenomenon of post-tetanic potentiation. Granit reported that high-frequency afferent stimulation left a state of post-tetanic potentiation in the spinal cord that greatly and persistently facilitated the stretch reflex (21). Since an exaggerated stretch-reflex is believed to be equivalent to spasticity, post-tetanic potentiation caused by excessive activity of the spindle afferents may be a contributing factor in spasticity.

Hyperactivity of the gamma system is not, however, the explanation for all types

of spasticity in the animal. Another kind of extreme extensor spasticity is obtained by anemic decerebration (4, 71) that destroys part of the cerebellum and pons. De-afferentation by sectioning of dorsal roots (58) did not alter this spasticity, nor did administration of chlorpromazine (31). In this state the spasticity must be maintained by pure alpha activity, as the gamma loop has been severed. In their experiments with cooling and removal of the anterior cerebellar lobe, Granit *et al.* (25) could show that the behavior of the spindles had changed from active to passive, and that this type of spasticity seemed to be maintained by alpha activity. Granit cautioned, however, against concluding that the two extremes—pure gamma activity and pure alpha activity—occur as mutually exclusive alternatives; he felt, rather, that there is an important linkage between the gamma and alpha systems for the execution of most motor acts, the cerebellum playing a significant role in the organization of this linkage.

Spasticity of spinal origin, so prominent in animals after temporary ischemia of the spinal cord, has been investigated by Gelfan and Tarlov (18). These authors stressed the importance of the influence of spinal interneurons on motoneuron activity, and proposed that their destruction denervates the motoneurons. The denervated motoneurons, like denervated muscle fibers, become hyperexcitable, to the point of discharging spontaneously; these spontaneous discharges are then directly responsible for the spasticity in the animal. McCouch and associates (53), on the other hand, concluded that exaggeration of reflex and spasticity in spinal animals is caused largely through replacement of degenerated descending tracts by sprouting from neighboring afferent fibers which may gain an abnormal degree of control of the reflex arc.

The application of the aforementioned studies to man is difficult, and it would be a crude simplification to translate indiscriminately to the human the findings from stimulation or ablation experiments in animals. Our knowledge of the pathophysiology of disorders of muscle tone in man is very limited, and only a few deductions can be made.

Spasticity: As a clinical problem, spasticity affects most severely the flexor muscles of the arm and the extensor muscles of the leg. Typical of this state is the presence of the so-called lengthening and shortening reactions. Against stretching of the muscle there is resistance initially, then a sudden giving way. Similarly, as the muscle is shortened it assumes tension at the shortened length. It has been shown in the animal that the lengthening or clasp-knife reaction is caused by the high-threshold inhibitory Golgi's receptors (a sensing device for muscle tension) in the muscle tendon (20), which provide an important safety mechanism against over-stretching of the muscle. Spasticity is accompanied by increase of muscle stretch reflexes and frequently by muscle weakness.

The similarity between upper motor neuron spasticity in man and decerebrate rigidity in the animal has been stressed repeatedly, and it seems to be generally appreciated that overactivity of stretch-reflexes constitutes the basis of spasticity. Matching demonstration in the animal, Förster (15) has shown in man that sectioning of the dorsal root alleviates spasticity temporarily. Liljestrand and Magnus (47) found that infusion of procaine into the triceps muscle of the decerebrate cat markedly diminished spasticity in this muscle, and their observation was confirmed by Bremer and Titeca (5). Walshe (72), conducting similar experiments in patients with spastic hemiplegia and parkinsonism, could abolish the abnormal tone and tendon reflexes without affecting muscle strength. It was inferred from these studies that spasticity was abolished by a blockage of proprioceptive fibers from the muscles.

Matthews and Rushworth (52), however,

confirming Gasser and Erlanger's (17) demonstration that small fibers were more sensitive to Novocain than were the larger ones, showed that it was rather a selective blocking of the small-diameter gamma motor efferents. In later experiments on patients with spasticity and parkinsonian rigidity, Rushworth (61) obtained additional support for this conclusion. By injecting dilute procaine near the sciatic nerve he could abolish clonus, resistance to passive displacement, and the ankle jerk without reducing the motor response. Observations by Nathan (55) and by Kelly and Gautier-Smith (38) that intrathecal injection of phenol abolished spasticity reinforce the suggestion of preferential blocking of fusimotor axons in the ventral roots. Landau *et al.* (44), by means of differential nerve blocks at intrathecal and epidural levels, confirmed and extended these observations.

Granit and Kaada's (26) studies of excessive gamma system activity in the decerebrate animal stimulated similar experiments in man. Asai and Hufschmidt (2) showed in spastic persons that innervation of the limb was maintained by the gamma system. During voluntary contraction of the spastic muscle, sudden relaxation of it (functional de-afferentation), leading to cessation of proprioceptive impulses from the muscle spindles, promptly interrupts innervation of the muscle. The spastic muscle seems to be under the influence of high-frequency proprioceptive impulses that keep it in a state of tetanic tension.

In a recent study of patients with muscle spasms and abnormal posture resulting from cervical spinal-cord lesions, Penry *et al.* (57) found a loss of interneurons and complete isolation of anterior horn cells, or both, in the segments responsible for innervation of muscles clinically in spasm. They concluded that destruction of the internuncial neurons, with preservation of collaterals from the dorsal column to motoneurons, favored release of stretch-reflexes. In the more severe cases, complete isolation of anterior horn cells, loss of interneurons within the anterior horn, and damage to the Renshaw cells (which are said normally to dampen motoneuron activity) resulted in continued discharge of impulses leading to muscle spasm.

Rigidity: Little is known of the pathophysiology of rigidity, and the new information raises almost as many questions as it answers. In man, muscular rigidity arising from disease of the basal ganglia is different from spasticity. It affects the flexors and extensors more nearly equally, and persists throughout the range of passive motion. Muscle length is adapted to every new limb position by maintaining muscle tension constant, and the phasic stretch reflexes usually are not increased. The augmented resting tension in the rigid muscles makes reflex elicitation more difficult, for the muscle spindles cannot be stretched quickly and extensively enough. If the rigidity is strong at the time of reflex elicitation, the reflex contraction is small. If, however, these mechanical factors are circumvented by eliciting the reflex electrically, reflex activity appears to be normal (67).

The absence of reflex exaggeration and the independence of rigidity from the degree of muscle stretch suggested that in parkinsonism there is a deficient activity of the central mechanisms for gamma innervation. Support for this hypothesis was adduced by Hassler (29) from the ineffectiveness in parkinsonism of Jendrassik's maneuver (a clinical procedure for reinforcement of reflexes, in which the patient hooks his hands together by the flexed fingers and pulls apart strongly), which normally throws muscle spindles into action by increasing the contraction of the intrafusal fibers. Further supportive (33, 64) evidence for deficiency of central gamma innervation was the occurrence of parkinsonism after chlorpromazine administration; this was in keeping with the observation by Henatsch and Ingvar (31) that spindle activity in the

animal was inhibited promptly after intravenous injection of chlorpromazine. Hassler suggested that, in parkinsonism, the lack of spindle innervation caused a suppression of the circuit-controlling muscle length, the muscle then being under the sole control of the circuit that regulated muscle tension (29).

Muscle rigidity seems to consist of two components. One is of reflex character, and Struppler, Struppler and Schwab (68) have attributed it to exaggeration in tonic-myotatic stretch reflex activity that resulted in an increase of the resting muscle tension. Hufschmidt (35) concluded that the inhibitory reflexes from Golgi's tendon receptors were hypersensitive in parkinsonism. In rigidity, voluntary activity of muscle is inhibited more strongly than in health; to achieve movement at all, tension has to be greatly increased, and this, in extreme cases, may result in complete inhibition of all motoneuron activity.

The central component of muscle rigidity appears to cause what has been referred to as "resting rigidity," which has been relieved by surgical interruption of certain subcortical structures. Since pathologic studies suggest that the substantia nigra is preferentially involved in parkinsonism, active central-muscle spindle innervation has been thought to depend upon this specific structure, from which the impulses travel via the nigro-reticulo-spinal system to the gamma neurons in the anterior horns of the spinal cord. This tract may be identical with the fast-conducting pathway in the lateral funiculus described by Granit and Holmgren (24).

Following deficiency of the nigro-reticulo-spinal system and loss of ascending collaterals from the nucleus niger to the pallidum, descending tonic pallidoreticular influences predominate, resulting in enhancement of myotatic reflexes (35). Inhibition of the gamma activity results in loss of the "starter function" of the gamma system and interferes with phasic muscle innervation and rapid beginning of movement, and with associated movements and many automatic movements as well (37).

It now seems that in the operative treatment of parkinsonism the procedure of choice is the interruption of the pallido-thalamo-cortical system before it reaches the cortex, from which point it would project via efferent pyramidal and extrapyramidal connections to the anterior horn cells, causing excessive discharges (30). Such a surgical approach spares the efferent tracts and thus avoids causing muscle weakness. This interruption abolishes the facilitory influences on the interneuronal-anterior horn cell mechanism, resulting in suppression of the enhanced myotatic reflexes; but it also suppresses descending impulses generating resting rigidity. In this connection, Ward (73) made the interesting observation that electric stimulation of the oral ventral thalamic nucleus resulted in inhibition of spindle activity.

CONCLUSIONS

Although the physiologic mechanisms for muscle tone, spasticity and rigidity have been elucidated only incompletely, certain general conclusions can be drawn.

Muscle tone develops on a reflex basis, and its most important regulating device is the muscle spindle. The afferents of these spindles enter the spinal cord and connect with the alpha ventral horn cells, from which impulses pass directly back to the muscle. This circuit has been called the gamma loop. A striking feature of this system is its constantly varying tonic permanent activity, which maintains facilitation of the ventral horn cells at all times. The spindle loop is under the constant influence of powerful supraspinal facilitory and inhibitory command, to which subcortical nuclei and reticular substance, as well as vestibular and cerebellar pathways, are of great importance. These supraspinal controls exert their influence upon the muscle spindles by way of the efferent gamma fi-

bers. In addition to support from the gamma system, muscle tone probably receives support from energizing discharges which impinge directly upon the alpha motoneurons by way of the corticospinal pyramidal fibers.

No specific center is responsible for states of exaggerated muscle tone. Both central and peripheral reflex connections participate in producing spasticity and rigidity.

In spasticity the gamma system seems to be extremely hyperactive, maintaining excessive excitation of the ventral horn cells, possibly by causing a state of post-tetanic potentiation in the spinal cord that greatly facilitates the muscle stretch reflex (exaggerated stretch reflex being equivalent to spasticity). The hyperactivity of the gamma system is brought about by a disturbance of balance of its central facilitory and inhibitory control. In addition, alpha activity may have a contributing effect in spasticity, as it does in the maintenance of normal muscle tone. Spasticity of spinal origin could be due in part to spontaneously discharging alpha motoneurons that have been denuded of the presynaptic terminals of their spinal interneurons, thus reducing integrative supraspinal influences.

The pathophysiology of rigidity is unknown. As in spasticity, both peripheral reflex and central components seem to play an important part. Deficient activity of supranuclear mechanisms for gamma innervation—possibly on the basis of a substantia nigra lesion—interfering with muscle spindle innervation and with the trigger function of the gamma loop has been proposed, and would explain the muscle stiffness and the inability of patients with parkinsonism to initiate quick movement. This difficulty may be aggravated by hypersensitivity of Golgi's tendon receptors that inhibit movement reflexly. In addition, nucleus niger lesions have been thought to have a releasing influence upon certain hypothetic neuronal reverberating circuits, which then produce a facilitating effect on the interneuronal mechanisms of the spinal cord and lead to excessive impulse-firing to the muscle. The alleviation of rigidity following surgical interruption of the pallidothalamo-precentral-spinal system suggests that this neuron chain plays a prominent part in maintaining rigidity.

It is obvious from this presentation that we are only at the beginning of the exploitation of a fascinating field. Anatomy has far outrun physiology, and at present no satisfactory theory concerning the clinical concepts under discussion can be formulated.

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SOME SOCIAL PSYCHOLOGICAL ASPECTS OF DELIVERY ROOM DIFFICULTIES[1]

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Several investigators have demonstrated the importance of emotional and sociologic factors during pregnancy and subsequent difficulties of both a somatic and psychologic kind (2, 5, 7). Such studies of socio-emotional factors in pregnancy have most frequently focused upon certain pathologic or otherwise deviant attributes insofar as they may be related to the experiences which are undergone during pregnancy, delivery, and the neonatal period (1, 3, 4). A major aim of this paper is to report some empirical relations which suggest that the extent of difficulties during delivery on the part of pregnant women may be a function of social psychologic characteristics which do not fit with a conception of the "pathological."

RESEARCH SETTING AND SUBJECTS

This is an ancillary study to the National Collaborative Study of Perinatal Factors in Cerebral Palsy and Birth Defects.[3] The collaborative investigation is a longitudinal study of factors related to the etiology of organic impairments in the neonate being conducted at 16 university-hospital centers throughout the country. Work reported by others (10, 11), as well as clinical work at the Providence Lying-In Hospital, one of the study sites for the collaborative study, suggests that the labor and delivery phases of pregnancy are crucial periods in the emergence of organic impairments.

The purpose of this ancillary study is to examine some social psychologic attributes and characteristics of women who regard pregnancy as an "illness" and who wish to act "as if" they were ill during pregnancy (6). A further aim of this investigation was to determine whether the pregnant woman's perception of herself as "ill" during the course of her pregnancy might be associated with subsequent difficult labor or delivery.

The subjects of this study were 94 expectant mothers who were interviewed at the time of their regularly scheduled obstetric visits. Sixty-two were systematically drawn from the clinic population of the Providence Lying-In Hospital; the remaining 32 were the patients of obstetricians in private practice.

PROCEDURES

Indices of Difficulty: Two major indices of difficult or complicated deliveries were used: length of labor time and gross complications.

Labor time was determined by means of data which were recorded for purposes of the Collaborative Study. Specifically, observers noted the time of the onset of contractions on standardized forms, and later noted the time at which the patient entered the delivery room. The lapse in time was regarded as a suitable indicator of length of labor time. These data were available on 87 of the 94 women—those whose pregnancy went uninterrupted to full term. The remaining seven patients underwent cesarean

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[3] The National Collaborative Study of Perinatal Factors in Cerebral Palsy and Birth Defects, operating under a research grant from the National Institute of Neurological Diseases and Blindness, is a long-range longitudinal study of factors related to the etiology of organic impairments in the newborn.

section; labor time data were not an adequate index in those cases, in spite of the fact that three of the seven had been in labor for varying periods of time before the decision was made to perform a section. An operational assumption of this study was that a lengthy labor time was a greater indication of difficulty during delivery than was a shorter labor time.

Second, indices of gross difficulties and complications were drawn from the regularly maintained records at the Lying-In Hospital. Notations were first made of any complications noted while the woman was in the labor room. These included such observations as unexplained bleeding, any fetal positions or movements which seemed to interfere with the course of labor, and so on. Notations were also made of the general condition of the woman in the labor room (*e.g.*, premature rupturing of the membranes, any abnormal heart murmurs or palpitations). Third, abnormal conditions in the delivery room itself were noted (*e.g.*, breech position of the fetus, abnormal fetal heart beat, extreme blood pressure changes on the part of the mother, and the like). Fourth, a record was kept as to the use of forceps: low, mid or high; and mild, moderate or heavy pressure needed. Finally, notations were made of any cord complications as to both the number of loops and their degree of tightness.

TABLE 1
Delivery Room Difficulty Data

Labor Time	Mean Labor Time: 10.2 (hours)	Range: 0.2–73.5
Gross Difficulties:		
Stillborn	2	
Interrupted Pregnancy	2	
Neonatal Death	1	
Posterior or Breech Position	3	
Mid or High Forceps with Pressure	6	
Abnormal Bleeding or Discharge	3	
Cord Complications	6	
Gravida or Fetal Distress Respiratory or Heart Difficulties	7	
Total Gross Difficulties	30	

The subjects were then divided into two groups: those women who had experienced a "gross abnormality" during labor or delivery; and those who had not—regardless of the length of labor time. There were 30 women in the "abnormal" group and 64 in the "normal" category. Those women whose labor had been interrupted for purposes of sectioning were placed in the "abnormal" group, while those whose section had been anticipated from the outset were assigned to the "normal" group. Two women on whom data were available were eliminated from the study. One had a long surgical history and died before coming to term; the other was a woman with a chronic kidney ailment. The kinds of data used for indices of delivery room difficulties are given in Table 1.

Social Psychologic Data: Each woman was interviewed by a sociologist. These interviews, ranging in length from 45 minutes to an hour and a half, have been described in more detail elsewhere (7–9). Data were gathered relative to several different areas. First, the extent of the woman's perception of pregnancy as an "illness," requiring one to act "as if" one were "sick" during pregnancy, was determined by means of a focused series of questions on the sick role. Specifically, the women who viewed pregnancy as an illness tended to (a) expect to be exempt from their normal social obligations during pregnancy, (b) desire to "get over the condition" as quickly as possible and return to a normal round of life, (c) feel obligated to be concerned about organic sensations and possible medical complications during pregnancy, (d) anticipate pain and suffering as natural corollaries of pregnancy, and (e) feel obligated to enact a subordinate role *vis-à-vis* their attending doctor. The responses to this interview

measure were scored on a 0 to 15 basis, with a higher score indicating greater perception of self as "sick" during pregnancy.

Certain classificatory data were gathered concerning social status factors, such as income, occupation and education, as well as age and the number of previous children.

Obstetricians who attended the women were also interviewed in terms of their own definition of the condition of pregnancy. By relating the woman's definition of pregnancy to that of the attending physician, we were able to derive an indicator of the presence or absence of latent conflict in the doctor-patient relationship.

These data were then related to the delivery room events, first with regard to length of labor time, and then for purposes of comparing the women who had experienced gross difficulties with those whose labor and delivery were relatively normal.[4]

RESULTS

Labor Time and Social Psychologic Variables: Relationships between length of labor time and the social attributes and characteristics taken into account are reported in Table 2.

The most striking relationship to be found in Table 2 is that between definition of pregnancy as an illness and the length of labor time. Specifically, the more a woman regarded herself as "ill" during pregnancy, the greater was the likelihood of a longer period of active labor. Thus, the woman who, early in her pregnancy, expressed a desire and feeling of obligation to act "as if" she were sick, tended subsequently to experience a greater extent of "illness"—as measured by length of labor time—than did the woman who did not regard pregnancy as a condition of illness.

[4] For purposes of computing Pearson correlation coefficients, the data on education, income and occupation were scaled from 0 to 5, with the higher scale position indicating higher income, longer periods of formal education and higher occupational status.

It is noteworthy that while the other correlations reported in Table 2 are not large enough to be statistically significant, a consistent trend among them is found in terms of factors relating to socio-economic status. Specifically, the factors of income, education and occupational status were found to be negatively related to length of labor time. That is to say, longer labor time seemed to bear some relation to lower socio-economic status, while shorter labor time was more frequently associated with higher socio-economic standing.

As can be seen in Table 3, there was no significant difference between the mean labor time of privately attended women and that of women from the clinic population. Moreover, while there was a modest difference in labor time between those women who had had previous pregnancies, and the primiparas, the difference was not outstanding. It is also of interest to note that there was no significant difference in the mean labor time of the women who had experienced earlier fetal or neonatal deaths as compared with the labor time of multiparas whose earlier pregnancies had not resulted in fetal or neonatal deaths.

A striking relationship reported in Table 3 is that between length of labor time and the presence or absence of incongruities and

TABLE 2

Length of Labor Time and Social Characteristics Product-Moment Correlation ($N = 87$)

Patient Characteristic	r	t
Definition of Pregnancy as "Illness"	0.27	2.59*
Age	−0.09	0.85
Previous Children	−0.05	0.46
Month of First Visit to Doctor	0.23	2.21†
Income	−0.10	0.94
Subjects' Education	−0.10	0.94
Husbands' Education	−0.14	1.23
Husbands' Occupation	−0.12	1.09

* $p < 0.01$.
† $p < 0.05$.

TABLE 3

Length of Labor Time: Differences Between Groups (Snedecor's F)

Groups Compared	Mean Labor Time (in hours)	F
Private Patients	8.50	
vs.		1.02
Clinic Patients	10.80	
Ss Define Pregnancy Same as Doctor	6.73	
vs.		9.32*
Ss Define Pregnancy Different from Doctor	13.70	
Primiparas	12.70	
vs.		2.01†
Mulitparas	9.00	
Previous Fetal or Neonatal Deaths	9.90	
vs.		1.55
No Previous Deaths	5.50	
Gross Difficulties	13.10	
vs.		4.56†
No Gross Difficulties	8.20	

* $p < 0.001$.
† $p < 0.05$.

latent conflict in the doctor-patient relationship. Specifically, the women defining pregnancy as an "illness," who were attended by a doctor who did not so define pregnancy, tended to have significantly longer labor times than did women defining pregnancy in basically the same terms as did their doctors. The reverse also held: women who did not define pregnancy as an "illness," who were attended by an obstetrician who did, tended to have longer periods of active labor than did women who had been involved in a doctor-patient relationship in which both individuals viewed pregnancy as an "illness." In fact, the mean labor time of the doctor-patient role-conflict subjects was over twice as long as that of the women whose doctor's perception of pregnancy was essentially the same as their own.

Finally, it can be seen in Table 3 that those women who had experienced a gross abnormality either during the period of labor or during the delivery of the child had a significantly longer labor time than did women whose labor and delivery had not involved such gross difficulty. Such a relationship might suggest that excessively long labor tends to increase the probability of the occurrence of a gross deviation of either a physiologic or a morphologic kind.

Attributes of Subjects in Doctor-Patient Conflict: Thus far, the data have indicated that women most likely to experience periods of long active labor were those (a) who conceived of pregnancy as an "illness" to the extent of feeling obligated to act "as if" they were sick, (b) of comparatively low socio-economic status, and (c) whose doctor-patient relationship had involved latent conflict or incongruity in terms of the definition of pregnancy as an "illness." It is of further interest to determine whether other differences might be noted between women whose view of pregnancy was different from that of the doctor's and those who were not in conflict with the doctor's definition of the condition.

Table 4 shows that the women who had been in a role definition conflict situation with the attending doctor reported a significantly higher mean perception of themselves as "sick" during pregnancy than did the women who defined pregnancy in essentially the same terms as did their obstetricians. In other words, the doctors were generally less likely to view pregnancy as an "illness" than were the patients. Thus, the woman motivated to act "as if" she were ill during pregnancy, was likely to find herself in a latent stress situation with her physician.

Moreover, those women whose definition of pregnancy tended to conflict with that of the doctor first consulted a physician concerning their pregnancies significantly later (well into the second trimester) than did those whose doctor-patient relationship did not involve a basic definition conflict. And while the difference was not large enough to be statistically significant, there was a ten-

TABLE 4

Characteristics of Patients in a Pregnancy Definition Incongruity Situation With Doctor (Snedecor's F)

Groups Compared	Mean		F
Incongruity	Sick Role:	6.80	
vs.			12.81*
No Incongruity	Sick Role:	4.46	
Incongruity	Husband Occupation:	2.14	
vs.			0.41
No Incongruity	Husband Occupation:	2.55	
Incongruity	Husband Education:	2.92	
vs.			0.00
No Incongruity	Husband Education:	2.92	
Incongruity	Subject Education:	2.63	
vs.			1.33
No Incongruity	Subject Education:	2.63	
Incongruity	Income:	2.26	
vs.			0.00
No Incongruity	Income:	2.32	
Incongruity	Prev. Pregnancies:	2.16	
vs.			1.29
No Incongruity	Prev. Pregnancies:	1.64	
Incongruity	Month 1st Visit:	4.46	
vs.			6.34†
No Incongruity	Month 1st Visit:	3.39	
Incongruity	Age:	25.0	
vs.			12.45
No Incongruity	Age:	25.2	

* $p < 0.001$.
† $p < 0.01$.

dency for multiparas to find themselves more frequently in a definition conflict situation with their doctors than were primiparas. There were no significant differences in terms of either age or the traditional social class factors of income, occupation and education.

SUMMARY AND DISCUSSION

This is a report of a study of some social psychologic attributes of pregnant women who experienced difficulties during labor and delivery. Length of labor time and indices of complications were used as measures of difficulties. The social psychologic data were gathered by means of a semi-structured interview which consisted of four basic parts: 1) the extent to which the woman viewed pregnancy as an "illness" and expressed expectations to act "as if" she were sick during pregnancy; 2) basic socio-economic data, such as education, income and occupation; 3) data indicative of her past obstetrical experiences; and 4) interviews with the attending obstetricians with a view to determining the extent of conflict or convergence between the patient's definition of the condition of pregnancy and that of the doctor.

The data yielded the following findings: 1) Women who regarded pregnancy as an "illness" tended subsequently to have significantly longer active labor than did women who did not view pregnancy as an

"illness." 2) There was a tendency for women of low socio-economic standing to have longer periods of labor than did women of comparatively higher social status. 3) Subjects who consulted a doctor late in their pregnancy tended to have longer labor times than did those who first visited a doctor early in their pregnancy. 4) Primiparas tended to have longer labor times than did multiparas. 5) Women who had been in a "pregnancy definition conflict" situation with the attending doctor tended to have much longer labor times than did women who had not. 6) Patients who first visited a doctor in the second trimester of pregnancy were more likely to view pregnancy in a manner different from that of their doctors than were women who consulted a physician in the first trimester. 7) Women who experienced gross physiologic or morphologic difficulty during labor or delivery tended to have longer periods of active labor than did women whose labor and delivery were otherwise comparatively normal.

Although the reported findings must be regarded as tentative, at least three of the relationships found warrant some emphasis and discussion.

There was a striking relationship between length of labor time and the extent of the woman's definition of pregnancy as an "illness." Those women who expected to act "as if" they were sick during pregnancy were also those whose labor times records bore out that expectation. It seems clear that as an anticipatory response phenomenon this is what might be expected. These women were actually enacting a certain kind of social role during their pregnancy with which a long and arduous labor is not inconsistent.

It was found that women whose definition of pregnancy differed markedly from that of the attending obstetrician had much longer periods of labor than did women whose doctor-patient relationship did not involve pregnancy-role incongruities. It may be reasonable to expect that a stress situation existing between doctor and patient—however latent—would be more likely to lead to greater psychosomatic disturbance than would treatment relationships in which there was greater congruity between the orientations of the doctor and the patient. And it might also be expected that the emergence of greater doctor-patient similarity of orientation would be more likely to take place when the period of treatment was longer. In keeping with this it was found that both longer labor time and a higher incidence of doctor-patient incongruity occurred among women who first consulted a doctor during the second trimester of pregnancy than with those who visited a doctor in the first trimester.

Of suggestive importance is the relationship between length of labor time and the incidence of gross physiologic or morphologic difficulties. The longer the labor time, the more likely the appearance of a gross complication of some kind. In his discussion of the etiology of birth injuries to the newborn, Schwartz (10) has characterized birth as "a brutal process." By this he refers specifically to the effect of each labor contraction upon the pressure transmitted to the fetus via the intra-uterine fluid. It may well be that the likelihood that this process results in birth lesions becomes greater as labor becomes more prolonged. If this is in fact the case, then whatever means—social psychologic as well as medical—by which this effect may be reduced is of potential practical importance.

In general, this investigation has demonstrated that women whose social psychologic attributes and behaviors appear to fall within normal limits can be differentiated in terms of the kind and extent of difficulties they experience during labor and delivery.

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A COMPARISON OF FLUPHENAZINE, TRIFLUOPERAZINE AND A PLACEBO IN THE CONTEXT OF AN ACTIVE TREATMENT UNIT[1]

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INTRODUCTION

In the ever-increasing flow of articles on drug research in which effects of a drug are compared to that of another drug and/or placebo, there is typically one important aspect overlooked. That is the impact that the research itself has had, the contribution it has made to the obtained results.

It has been emphasized by Sherman (8) that psychopharmaceutic research must properly deal not only with the drug but also with the situation variables and the personal factors of the patient's response to the drugs. As Sherman points out: "It is necessary to specify the total social situation in which any drug experiment is carried out." Additional evidence of the importance of the attitude of the staff treating the patient has been amply attested to by Feldman (3) and Thorpe and Baker (11).

In this study an attempt was made to maximize the interest and involvement of the Unit Staff and to assess the efficacy of two phenothiazine tranquilizers (fluphenazine and trifluoperazine, hereafter referred to respectively by their trade names, Permitil and Stelazine) and a placebo.

The study was conducted on Fairway Unit of the Utah State Hospital. Virtually all of the unit's personnel was involved: the psychiatrist, psychologists, nurse, attendants and rehabilitation specialists.

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METHODS AND PROCEDURES

The first step was to establish an average or typical dosage for the new drug, Permitil. The cooperation of the patients was solicited; they were told that the staff would like to obtain their help in evaluating the effects of this new tranquilizer. Varying dosages were used on the participating patients until the staff was satisfied as to what constituted, on the average, the optimal dosage for the majority of the patients. The dosage selected was 5 mg. per 24-hour period. This was administered in two dosages of 2½ mg. at 7:00 in the morning and 12:00 noon. Based on our own impressions as well as on available information as to the relative potency of Permitil and Stelazine (1, 9), it was decided that Stelazine should be given in strengths two and one-half times that of Permitil, or at the dosage of 12½ mg. per 24-hour period. This also was to be administered in two dosages of 6¼ mg. at the same time as indicated for Permitil. A milk-sugar placebo was used as the non-reactive agent. All medications were prepared in capsules of identical appearance.

Selection criteria for subjects was one year's residence in the hospital and a diagnosis of a functional psychosis. This eliminated the acute, psychotic new patients and the clearly organic patients. Application of these criteria cut the available population in half. In addition, a few patients were excluded because they were discovered during the dosage-determining phase to have an unsatisfactory tolerance for Permitil. Fifty-six patients (43 per cent of the total number of patients on the Unit) qualified as subjects.

The patients who volunteered were then randomly divided into four experimental groups. A modified randomized-block method was utilized by reference to a table of random numbers, with the participating patients on each ward being randomly assigned to the four groups. This resulted in having approximately the same number of patients from each ward in each of the four groups. There were in all 41 men and 12 women, ranging in age from 20 to 60 years, with a mean age of 41.9 years. The length of hospitalization ranged from one to thirty years (mean, 13.7 years). All patients were diagnosed within the various categories of schizophrenia. After they had been assigned to their respective treatment groups, three of the fifty-six declined to cooperate in the experiment. In addition, it became necessary to drop four others from the project for management, medical and discharge reasons. Fortunately these four patients and the three who declined to participate were distributed among the four experimental groups. It was thus possible to simply drop randomly one more patient from the fourth group, leaving 12 subjects in each group.

For three weeks prior to the investigation, all medication was discontinued. At the end of this three-week period, and each of the subsequent two-week treatment periods, the participating patients were observed and rated by the unit personnel. The rating devices used were of two types: 1) the MACC, and 2) a clinical evaluation sheet which evaluated the patient's mood, interest in the activities of the ward and hospital, and changes in his work performance, behavior and attitude on and off the ward (Figure 1).

EXPERIMENTAL DESIGN

Table 1 illustrates the experimental design used. It should be noted that medications B and D were placebos, while medication A was Permitil and medication C was Stelazine. During the experiment all medications were labeled as A, B, C or D, with only the hospital pharmacist having knowledge of what medication was being given to each patient. Two placebo groups were used in order to diminish any overlay effects of one drug treatment to another, and to give both of the drugs an equal chance to show

Type of Rating: Industrial_____ Head Attendant_____ Rehab. Therapist_____
Doctor_____ Nurse_____

This evaluation is to be done by Friday_____. Describe your impression of the patient's behavior on the items listed below during the past two weeks as compared to his behavior (and your rating) during the two weeks just previous. After comparing the patient's behavior on the items below, please make a rating of his overall change in behavior during this two-week period as compared with the previous two-week period.

1. *Does he seem to talk more easily with others than he did two weeks ago?*
Much improved_____ Some improved_____ No change_____ Some worse_____ Much worse_____
Example of change:
2. *Does he seem more interested in doing things on and/or off the ward than he did two weeks ago?*
Much improved_____ Some improved_____ No change_____ Some worse_____ Much worse_____
Example of change:
3. *Does he seem to be in better spirits than he was two weeks ago?*
Much improved_____ Some improved_____ No change_____
Example of change:
4. *Does he seem more interested in others than he was two weeks ago?*
Much improved_____ Some improved_____ No change_____ Some worse_____ Much worse_____
Example of change:
5. *Does he seem more interested in his appearance than he did two weeks ago?*
Much improved_____ Some improved_____ No change_____ Some worse_____ Much worse_____
Example of change:

Overall Change in Behavior

Much improved_____ Some improved_____ No change_____ Some worse_____ Much worse_____
Additional Observations:

FIG. 1. Specimen of Clinical Evaluation Sheet.

TABLE 1

Experimental Design Showing Order of Drug Presentation

	Pre-Drug: 3 Weeks	Phase 1: 2 Weeks	Phase 2: 2 Weeks	Phase 3: 2 Weeks	Phase 4: 2 Weeks
Group 1	No Medication	Drug A Permitil	Drug B Placebo	Drug C Stelazine	Drug D Placebo
Group 2		Drug B Placebo	Drug C Stelazine	Drug D Placebo	Drug A Permitil
Group 3		Drug C Stelazine	Drug D Placebo	Drug A Permitil	Drug B Placebo
Group 4		Drug D Placebo	Drug A Permitil	Drug B Placebo	Drug C Stelazine

their effects after a patient had been through a placebo phase. In addition, it was thought that such a design would help give more valid ratings, because during any given two-week period there were patients receiving all four treatments. In other words, the design intrinsically helps to mask the treatment the patient is undergoing. It was, of course, impossible in some instances to conceal when the patient was on a placebo and when he was on one of the two medications because of side reactions or marked changes in behavior. It should be emphasized that all ratings were made independently, so that there was a minimum of contamination from one rater to another.

It will be noted from Table 1 that the ratings were all due on a particular date for each of the four treatment phases and the pre-drug period. The MACC's and the evaluation sheets were filled out by each head attendant for the patients on his ward who were participating in the experiment. A clinical evaluation sheet was also filled out by the psychiatrist, nurse, and the rehabilitation therapists. The Taylor Manifest Anxiety Scale was given by the afternoon attendants to those patients on their wards. Table 2 summarizes the ratings and tests used and by whom they were completed.

TABLE 2

Ratings and Tests Used in the Study

Test or Rating	Completed by:
1. MACC	Head attendant
2. Evaluation Sheet	Psychiatrist Nurse Head Attendant Rehab. Therapist
3. Taylor Manifest Anxiety Scale	Patient*
4. Overall Composite Evaluation	Psychologist

* In some cases the patient was aided by an attendant.

All ratings except the overall composite evaluation were made by various personnel and based on their evaluation of the patient's performance observed over each two-week treatment period. The composite rating made by the psychologist, however, was based only on the above-mentioned ratings. (The psychologist making the composite rating did not observe the patients.) There was no attempt to make this overall composite evaluation a quantitative reflection of the four individual ratings; rather, the psychologist assessed what each rater had said about the patient, as well as the rating given, and then made the overall or global rating based on these factors. In addition to the various rating scales that were completed on the patient, complete blood counts and urine analyses were made every other week just before a change in

medication was effected. The information from these analyses was used to guarantee that the drug had no toxic effect.

Following the completion of the fourth phase the data were tabulated and analyzed by an analysis of variance of: 1) the MACC ratings, 2) the overall composite clinical evaluation of all of the patients, 3) a correlation between the Manifest Anxiety Scale and the overall clinical composite evaluation; and 4) inter-rater correlation coefficients. In addition to the quantitative material, clinical or qualitative impressions were also gained prior to, during and subsequent to the experiment proper.

RESULTS AND DISCUSSION

QUANTITATIVE FINDINGS

The method used to analyze the data was the pseudo-triple analysis of variance method referred to by McNemar (6) as "Case 17." Since there were no specific hypotheses at the beginning of the research (other than the null hypothesis: that the effects of the treatments would be equal), a method was needed to determine which of the treatments were equal and which were different when the analysis of variance showed that there was a difference among the treatment means. Duncan's New Multiple Range Test (5) was thought ideal in such a case, because the hypotheses and formulations developed with benefit of the results suggested by the more global analysis of variance would not inflate the differences in the treatments.

Drug Comparison Using the MACC as an Index of Change: The MACC was used in this study for two purposes: 1) to reflect any behavioral changes that might have occurred over a two-week treatment period, and 2) to give some idea of the behavioral homogeneity in all four experimental groups. Table 3, which shows the pre-treatment mean MACC scores for the four experimental groups, indicates that there is a very small and non-significant difference among the mean scores. This suggests that the groups were indeed quite homogeneous.

An analysis of variance was carried out on the MACC scores, but this did not point to any significant differences due to any of the treatments.

The assessment of the whole picture of the patient and the changes he may have undergone due to one or another treatment was accomplished by combining changes in the MACC and the clinical evaluation sheets, which were filled out by the Unit staff, and the Taylor Manifest Anxiety Scale, which was completed by the patients themselves.

Table 4, which summarizes the analysis of variance of the overall composite evaluation, indicates that there was a significant difference at the .01 level among the drug treatments. By referring to Table 6, which is a summary of the multiple range effects, we have a comparison of the various combinations of differences which can occur. For example, when comparing Stelazine with Placebo B we find a mean difference of .52 between the combined ratings of the pre and post periods. This difference is significant at the .01 level. This suggests that Stelazine as a therapeutic agent is more efficacious than a placebo. When Stelazine is compared with the second placebo the same results are noted; that is, Stelazine is again superior to a placebo. Scanning downward to a comparison of Permitil and a placebo we note that the differences in Permitil as compared to the placebo are in favor of Permitil. That is, its effect seems to be similar to Stelazine in ameliorating the patient's symptomatology as compared with

TABLE 3

Pre-Treatment Mean MACC Scores for the Four Experimental Groups

	Group 1	Group 2	Group 3	Group 4
Mean Scores	47.6	48.2	46.9	47.0

TABLE 4

Summary of the Analysis of Variance for the Combined Ratings of Psychiatrist, Nurse, Attendants and Rehabilitation Therapists

Source of Variation	df	Square	F	Conclusion
Drug Treatments	3	3.3	9.16	Significant at 0.01 level
Sequence	3	2.93	1.05	Not significant
Drug Treatment × Sequence	9	0.56	8.13	Significant at 0.01 level
Within Subjects	44	2.78		
Residual	132	0.36		
Total	191			

TABLE 5

Summary of Duncan's Multiple Range Test for Differences Due to Drugs Based on the Combined Ratings of Psychiatrist, Nurse, Attendants and Rehabilitation Therapists

Number of Means Compared	Differences	Difference	Shortest Significant Range		Conclusion
			0.05	0.01	
4	Stelazine-Placebo B	0.52	0.261	0.338	Sig. at 0.01 level
3	Stelazine-Placebo D	0.44	0.254	0.328	Sig. at 0.01 level
2	Stelazine-Permitil	0.04	0.239	0.314	Not significant
3	Permitil-Placebo B	0.44	0.254	0.328	Sig. at 0.01 level
2	Permitil-Placebo D	0.36	0.239	0.314	Sig. at 0.01 level
2	Placebo D-Placebo B	0.08	0.239	0.314	Not significant

"merely giving him a pill," which has no pharmacologic effect.

When the combined ratings are used to determine whether Permitil is a more efficacious therapeutic agent than Stelazine, the data in Table 6 indicate that the two do not differ significantly in their therapeutic effect. Nor do the two placebos, B and D. In summary, the drugs Permitil and Stelazine seem to have a pronounced advantage over a placebo in improving the patient's behavior by reducing his symptomatology and maximizing his adjustive capacities.

Inter-Rater Agreement: Table 6 presents the product-moment inter-rater correlation coefficients and the rater correlations with the overall composite evaluation. It is seen that the inter-rater correlations are quite low, ranging from .19 to .27. The nurse and the attendants have the highest index (.27), followed by the attendants and the rehabilitation staff (.25). The results suggest that the raters, by and large, were sampling different aspects of a particular patient's behavior. The fact that all the coefficients are positive indicates that there is a concordance of opinion regarding the direction of change—improvement or regression.

Taylor Manifest Anxiety Scale and the Overall Composite Evaluations: The correlation between self-reported anxiety based on raw scores from the Taylor Manifest Anxiety Scale and changes in the be-

TABLE 6

Inter-Rater Pearson Product Moment Correlation Coefficients

	Rehab. Therapist	Attendant	Nurse	Psychiatrist
Psychiatrist	0.19	0.17	0.25	
Nurse	0.19	0.27		
Attendant	0.25			
Overall Composite Evaluations	0.35	0.46	0.47	0.80

havior of the patient based on the fine-point overall composite evaluation was computed. The correlation of −.17 (significant at the .05 level) indicates that anxiety and adaptive behavior are inversely related. This is a rather widely accepted point of view (4, 6, 10) and is commensurate with our original assumption. Hence we would expect that reports of anxiousness and discomfort by the patient would tend to occur in relation to behavioral abnormalities; by the same token reports of "peace of mind and body" would tend to go along with more adjustive behavior. There is one mitigating factor in that some patients were not able to complete the scale. The partial results, however, can be interpreted as demonstrating the relationship between anxiety and adjustment for patients that could be so measured.

Side Reactions: Along with the beneficial results observed, there was a fairly high proportion of side effects—especially of the extrapyramidal sort.

Tables 7 and 8 indicate the extent of side effects of Permitil and Stelazine. There is no reason to expect a greater number of side effects in one drug as against the other, nor were significant differences found. (Patients receiving placebos showed no side effects, and at no time during the study were there any toxic effects as indicated by the urine analyses and complete blood counts.) It was felt, however, that there were some definite differences in the kinds of side effects produced by Permitil and Stelazine—especially in the time of onset. These will be discussed in the following section.

TABLE 7

Summary of X^2 on Occurrence of Side Effects vs. No Side Effects Produced by Permitil and Stelazine

	Side Effects	No Side Effects	Total
Permitil	17	31	48
Stelazine	19	28	47
Total	36	59	95

$X^2 = 0.25$, $p = 0.50$–0.70.

TABLE 8

Summary of X^2 on the Severity of Side Effects Produced by Permitil and Stelazine

	Moderate	Severe	Total
Permitil	10	7	17
Stelazine	15	4	19
Total	25	11	36

$X^2 = 1.71$, $p = 0.30$–0.50.

QUALITATIVE FINDINGS

During and subsequent to the experiment proper, numerous clinical impressions were gathered. Most prominent among these was the apparent ability of Permitil and Stelazine to effect changes in a patient's behavior and attitude. These included loss of apathy, increased interest in the environment, a better ability to interact and communicate, all of which were characteristically lacking in the schizophrenic patients prior to the study. Perhaps the one symptom common to all of the patients included in the project was that of apathy, with its resultant unresponsiveness to the environment, to other patients and to the therapist. Changes of an alerting nature were noted in many of our subjects. The changes observed seemed to take place in the following order: 1) increased affective responses away from apathy, 2) improved quality and quantity of communication, and 3) increased alertness and awareness of the environment.

These changes were noted to occur within five to seven days following initial drug administration. It was assumed that both Permitil and Stelazine were potent drugs, capable of exerting their effects in a relatively short period of time. This seemed in fact to be the case, as demonstrated by the rapidity of the drug action and the large number of behavioral changes occurring within a two-week period on either of the two medications. In itself this is important because it may provide a better index of the drug effect as compared to the milieu effect. The latter is difficult to determine

where an evaluation period extends over weeks or even months on a single drug (2).

One impression gained from this study was that intermittent periods of no pharmacological stimulation and consequent activation by one of these two potent drugs may in itself have value; a kind of "jarring effect" may occur. This particular effect was noticeable during the placebo period in several of the patients, who were more communicative and responsive to questions than they had been prior to the study. It was as if the patients, during the activation period of the medication, had been able to break the barrier of silence between themselves and others with some degree of satisfaction, and were willing to experiment further even when the drug was withdrawn.

Side effects observed in the drug phases of the project were almost exclusively of the extrapyramidal variety. In order of frequency they were: 1) akathisia, 2) dystonic reactions, and 3) parkinsonian symptoms. One or the other of these neurological symptoms occurred in 38 per cent of our cases; an additional 12 per cent showed increased restlessness. Side effects were much more common in the female subjects, occurring in seven of the ten women in the project. There were a few somatic reactions as well, such as fatigue, anorexia, dry mouth, dizziness and headache. None of these somatic reactions, however, persisted or caused sufficient distress on the part of the patient to have him ask for relief.

The question should be raised whether the neurological or side effects are not in some ways desirable. Side effects are commonly held to be unwanted, not useful, deleterious or harmful—to be avoided at all costs. Yet in some cases the extrapyramidal symptoms appeared to be useful indicators of therapeutic potential. Some of the patients who developed side effects were also those who improved in their overall behavior and in their overall responsiveness to the staff and to the other patients.

It is suggested that the therapeutic zone of a drug that is likely to change the target symptom of apathy, is very close to the extrapyramidal zone in a certain number of patients. It was found that the extrapyramidal symptoms could easily be managed by the use of Akineton, 2 mg., with the occasional addition of sodium phenobarbital, 1½ grains. In only three instances was it found necessary to resort to temporary reduction of the medication.

The two drugs are considered to be very useful, and yielded roughly equivalent improvement rates. The clinical impression, however, was that Permitil was the more definitive of the two, producing changes that were more clear-cut. Typically this was reflected in the patient's aptness at subjectively describing a like or a dislike of the affect he was experiencing when taking Permitil as compared to Stelazine. Permitil seemed to have a more stimulating effect, often producing a feeling described as wanting to do more things and feeling more like talking. In retrospect it would seem that the patients on Permitil tended to talk about their delusions and hallucinations more freely.

Permitil produced the neurological effect on the average of five days after the start of medication, whereas Stelazine produced the same side effects on about the eighth day. Patients on Permitil often described a feeling of well-being during the morning, even though the afternoon was characterized by akathisia-like symptoms; they often expressed a preference for the drug.

Of the two drugs, Permitil was the more potent in terms of milligram dosage. It appeared that overall changes, away from apathy and lack of initiative, were more clearly and easily determined. At the same time it is suggested that Permitil may require more attention, adjustment in dosage and management of stimulation, than does Stelazine.

Continued use of the two drugs has indicated that the dosage of Permitil which will effect improvement without producing dis-

tressing side effects ranges from two milligrams to four milligrams for women and from four milligrams to eight milligrams for men. These values seem to represent the average maximum dosage in the majority of our patient group.

SUMMARY AND CONCLUSIONS

This study was conducted to assess the efficacy of two drugs and a placebo on a homogeneous group of schizophrenic patients in a hospital setting. A secondary goal was to maximize the participation of the treatment team.

Optimal dosages for the two drugs, Permitil and Stelazine, were established, and these drugs and a placebo were administered in four pre-arranged orders to four groups of 12 patients who had been randomly assigned to the treatment groups. The order of treatment was so arranged that a placebo would always fall between two of the drug treatments. At the end of each of the four treatment periods rating scales and psychological tests were compiled for each of the 48 patients in the study. Based on these data, an overall composite evaluation was made. Both quantitative and qualitative aspects of the ratings were taken into consideration to make the final rating. In addition, urine analyses and complete blood counts were made on each patient every other week preceding a change in medication.

Statistical treatment of the data included: 1) analysis of variance of the drug effects, and 2) correlations among the raters, between each of the raters and the overall composite evaluation, and between a test of anxiety and the degree of adjustment manifested by the patient. Qualitative findings were also collected and reported.

The following conclusions can be drawn:

1) When the MACC was used alone as a gauge for measuring improvement or intensification of symptoms due to placebo and drug treatments, there were no differences noted.

2) When the MACC scores were considered in relation to clinical evaluation sheets made out on the patients by each member of the staff and the Taylor Manifest Anxiety Scale, there were no significant differences between Permitil and Stelazine in terms of the behavioral improvement that they elicited. The two drugs differed significantly from the placebos in ameliorating the symptoms of the patient.

Both drugs were felt to contribute to changing the symptoms of apathy in the direction of increased communication and interest in the environment. All patients in this study had been refractory to drug treatment in the past, but alternation of the drug and placebo treatments tended to attenuate their characteristic apathy. These changes were rapid and were noted to occur within five to seven days in the majority of cases. It was suggested that changes away from apathy, even with an increase in management problems, represented therapeutic progress.

Permitil and Stelazine showed essentially the same overall improvement rates, but Permitil seemed to have a more definitive effect; it produced changes that were more clear-cut. Of the two drugs, Permitil was the more potent in terms of milligram dosage; fewer or smaller pills could thus be given. (In the case of many patients this is a psychological advantage.)

3) Low correlation coefficients among raters may reflect the fact that raters were concerned with different aspects of patient behavior.

4) A correlation between the patient's reports of his anxiety, and improvement, suggested that within measurable limits anxiety and adaptive behavior are inversely related.

5) Side effects were evaluated. None of the patients developed side effects when given a milk-sugar placebo. On the other hand, 38 per cent of those patients receiving the active medications had concomitant side effects. There were, however, no sig-

nificant differences found in the severity or number of side effects produced by the two drugs. Side effects were almost exclusively extrapyramidal, *i.e.*, akathisia, dystonic reactions and parkinsonian symptoms, but at no time during the study were toxic effects noted. Onset of side effects was more rapid for Permitil than for Stelazine, averaging five and eight days respectively. It was suggested that side effects which are usually given an undesirable connotation can be useful to the clinician as an index of therapeutic potential.

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A PROCESS STUDY OF THE EFFECTS OF DEPROL ON DEPRESSION[1]

Exemplification of a Method of a Psychodynamic Process Study of Psychotropic Drugs

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The evaluation of tranquilizers and other psychotropic drugs shares the methodological difficulties of the evaluation of other therapies, especially psychotherapy. Outstandingly the difficulties in this latter field have been the criterion problem, the problems involved in the fact that there may be spontaneous changes over a period of time, and the impossibility of matching patients. The double-blind method of study of an experimental group and a control group has met some of these problems in what must be called "outcome" studies; in essence, they provide a chance to compare patients before and after, but such studies fail to answer the essential nature of the changes in a given patient, the psychodynamic factors affected by the drug at the time of the changes.

In order to deal with these problems we designed a process study concerned with the psychodynamic effect of one particular drug, Deprol,[3] recommended for the treatment of depressions.

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[3] Trademark by Wallace Laboratories. Chemical formula: meprobamate (400 mg.), benactyzine hydrochloride (1 mg.).

The crux of our method is predicated upon the fact that we were not interested only in variables such as "degree of depression" but wanted to anchor our appraisals in specific psychoanalytic hypotheses with regard to depression, *viz.*, that depression involves a great deal of orality, intra-aggression, a severe super-ego, narcissism, and low self-esteem (3).

The method of study emphasizes a multiplicity of ways of gauging the drug effect: on the conscious, preconscious and unconscious level, by studying manifest behavior, projective data and dreams; by methods of appraisal of different dimension, namely clinical rating by clinical interviews and observation by nurses, as well as dynamic study of Thematic Apperception Test (TAT) materials and dreams, and also by study of TAT material by the semantic method of content analysis. The ratings deal mostly with the conscious level, the TAT with preconscious, and some unconscious material and the dream with the unconscious level.

Aside from different levels and forms of study, it is an important feature of the design to attain measures not only before and after the drug, but study the process of dynamic change by measurements along the course of treatment.

The advantages of most of the features mentioned above are, we submit, obvious. The advantage of gauging preconscious and unconscious changes aside from manifest ones may need a word of explanation: the theoretical interests in studying the relationship of the variables mentioned to depression, and the effect of the drug on the one or the other or all of them is easy to see. Surprisingly, the practical importance of the

dynamic appraisal of latent factors seems less apparent: that many a depressed patient may improve manifestly without doing so latently, and then commit suicide. The clinical dictum that depressed patients become most suicidal after some manifest improvement has some basis in the assumption that only when the worst lethargy has worn off are they "energetic" enough to harm themselves. (In a more sophisticated view, one might see a deep depression as a partially successful defense against aggression, which when interfered with, may lead to intra-aggression.)

Most important in this context is that certain drugs may well produce manifest improvement without a really dynamic change. Dexedrine and its derivatives may serve as an outstanding example of this possibility: very often a superficial elation is produced which may not stand at all in the way of a suicidal act. On the other hand, it is likely that if a drug can be shown to do away with the dynamic forces responsible for the depression, a more genuine result, safer clinically, has been achieved. By tapping the preconscious as well as the unconscious level, we are in a better position to appraise "real" change as opposed to "superficial" change: we are in a better position to predict manifest behavior of patient as the depression lifts.

With regard to both the process-approach and its inter-relation to specific psychodynamic hypotheses, this study is related to earlier explorations of the psychoanalytic process by the senior author (2).

In effect we set out to test whether Deprol will, indeed, decrease depression and whether this decrease, if it occurs at all, will also manifest itself in any or all of the variables hypothetically related to depression. Because the two component drugs of Deprol are also considered anxiety-relieving ones, we included scales concerning this variable.

It is possible, then, that we could find that Deprol would have no effect whatsoever on patients and could not significantly be differentiated from the placebo—or it could turn out that the patients were clinically judged manifestly less depressed, or not depressed, but showed no changes in TAT or dreams. It might also develop that they would show changes in some of the variables and not in others, or show changes in their conscious functioning and in the relatively preconscious ones that appear in the TAT, but not show changes in their dreams.

It was also possible that the two drugs in Deprol might bring about a reduction of anxiety without showing any effect on any level with regard to depression. Ideally, however, one could postulate that an anti-depressant would lead to changes observable clinically, as well as on the level of the TAT and that of dreams.

PROCEDURE

SUBJECTS AND METHOD

The subjects selected were patients who either were admitted as in-patients to the psychiatric wards or were seen in the outpatient clinic at the City Hospital at Elmhurst. They were admitted to the study if they presented symptoms of depression regardless of their functional diagnosis, were between the ages of 21 and 65 years, showed no history of organic brain disorder, had not had ECT, were not alcoholics nor drug addicts, and had not received tranquilizing medication for at least one month prior to the study.

The subjects selected were randomly assigned to either the experimental or control group. Once selected in the study the patients were not given any other form of therapy.

PROCEDURE

The basic procedure involved the collection of three types of data:

1. *Clinical ratings* based upon periodic interviews by psychiatric staff. For each interview two staff members were present, completing their rating scales independ-

ently. Each patient had the same two interviewers throughout the study. The rating scales were designed to yield data with respect to anxiety and depression.

2. *TAT.* Immediately following the clinical rating the patient was given cards 1, 2, 3BM, 11, 12MF, 14 and 15 by the research psychologist. Each patient wrote his own responses on a form designed for this purpose. This procedure was based upon Eron and Ritter's findings (5) that written TATs yield the same quality and amount of thematic content as orally administered TATs. Bellak's (1) instructions for the TAT were used.

3. *Dreams:* A special form was presented each morning by the nurse to the in-patients on the study on which to write their dreams. The out-patients were asked to write their dreams each morning and to bring them in when they came for their next interview and TAT.

Much experimentation and variation in time interval and frequency of rating and testing, as well as with the kinds of rating scales to be used, was conducted during two pilot studies before the final investigation was begun. These studies are briefly described below, together with the major problems encountered.

Study # 1: The patients selected for this study were randomly divided into two groups. One group received Deprol (benactyzine and meprobamate, Wallace) 400 mg. QID, while the other group received a placebo during a period of six weeks. During this period each patient was interviewed and given the TAT, as previously described, once a week. Dreams were collected daily and an attempt was made to have the ward nurse fill in a rating scale daily on the behavior of each patient in the study. The scale used was provided by the Wallace Laboratories. In carrying out this study we found the patients to be very resistant to the TAT at such a short interval as one week and for six consecutive weeks. In addition there was the problem of the patients' remembering their responses from week to week which, although it would occur in both the experimental and control groups, would tend to obliterate any changes that might be due to drug effect. We found that some patients reported dreams while others did not, leaving us with meager data in this area. In addition we found it impossible to get daily ratings from the nurses, as well as to obtain satisfactory agreement among the nurses in their evaluations. Since the nurses were not on regular schedules we could not get the same nurses to rate the same patient throughout the study. Additional problems developed with the rating scale used by the interviewers: a low level of reliability was found. Early in this study we noted clinically that the effects of Deprol were very weak, and recognized the possibility that attempts to measure dynamic change in the patient would show little if any difference. But at the same time it was felt we might be pre-judging Deprol if we did not continue to acquire a reasonable sample.[4] We attempted, therefore, to make changes in the second study to overcome the problems encountered in the first.

Study # 2: The patients in this study were divided into two groups, as before, and given placebo or Deprol. They were interviewed and given the TAT in the same manner described above, together with the daily collection of dreams. The nurses' behavior rating scale was eliminated. The interviews and TAT were given at the outset, again after four weeks and, finally, after eight weeks.

During this study data collection was begun with 32 patients, but completed with only six, giving us a drop-out rate of more than 80 per cent. This was, variously, the result of some out-patients' failing to keep appointments because of the long interval between meetings; some improved sufficiently to go back to work; some got worse and required hospitalization. Among the in-pa-

[4] The senior author has had occasional good empirical results with Deprol on patients under close supervision, when the effects did not *seem* to be those of a placebo.

tients, some became agitated and required additional treatment which disqualified them; others were taken from the hospital by the family. Apart from this, some of the clinical staff felt strongly that a two-month period was too long to keep a patient on either Deprol or a placebo without any additional treatment.

Nothing could be done about the unevenness in reporting dreams. Reporting them in writing was clearly disadvantageous, but a lack of personnel made it impossible for this material to be collected orally, as we would have preferred. The clinical rating scale was modified and a number of training sessions were held with respect to its use; this yielded a higher degree of agreement.

The scoring system for the TAT was variously modified, and in each instance this resulted in low reliability between two scorers, requiring further development.

The third and final study constituted an additional attempt to refine our methods and procedures to overcome the problems described above.

Study #3: A group of patients was selected as described earlier and randomly placed in either an experimental group or a control group. These patients were interviewed, tested with the TAT and checked daily for dreams as outlined before. The interviews and tests were given at the outset, two weeks later, and again after four weeks. During the first two weeks the patients in both groups were given the placebo, while in the second two week period the patients in the experimental group were given Deprol and those in the control group continued on the placebo. The purpose of starting all patients on placebo, rather than starting some on the drug and then changing to placebo, was to avoid a possible carry-over effect of the drug. This design was also selected so that each patient would serve as his own control.[5]

The drop-out rate in this study decreased sharply (to 26 per cent) in comparison with the previous study, presumably as a result of the shorter interval. Further development of the TAT rating scale proved successful and as a result, we achieved adequate reliability. (See Table 1.) The clinical ratings also proved acceptably reliable. (See Table 2.)

Because of the unevenness of the dream material collected we were not able to subject it to meaningful analysis.

TABLE 1

Inter-Judge Reliability Between Four Judges of TAT Protocols Using The Elmhurst TAT Rating Scale

Scale Category	W*	Level of Significance
Depression	.85	.01
Anxiety	.61	.05
Hostility	.86	.01
Superego	.63	.01
Passivity	.70	.01
Narcissism	.83	.01

* Kendall's W. Siegel, S. *Nonparametric Statistics for the Behavorial Sciences*, pp. 229–239. McGraw-Hill, New York, 1956.

TABLE 2

Reliability Between Two Judges of Clinical Ratings of Patient Behavior Using The Elmhurst Clinical Rating Scale

Scale Category	Rho	Level of Significance
Depression*	.75	.01
Anxiety†	.73	.01

* This is item #6 on the scale, one rating of the global impression of feelings of depression.

† This is the sum total of items #1, 2 and 3 on the scale, which are concerned with manifest physical symptoms of anxiety, the patient's feelings of anxiety and an inference of the existence of anxiety which the patient has not verbalized.

RESULTS

Twenty-six patients, equally divided between experimental and control group, com-

[5] Following a suggestion by Dr. S. Chassan (4), then Chief, Statistical Service, St. Elizabeth's Hospital, Washington, D.C., consultant to our statistical and methodological problems. (Dr. Chassan is now at the U. S. Health, Education and Welfare Department, Office of Education, Washington, D.C.) He also suggested a specific method of statistical analysis of one case as useful for the future.

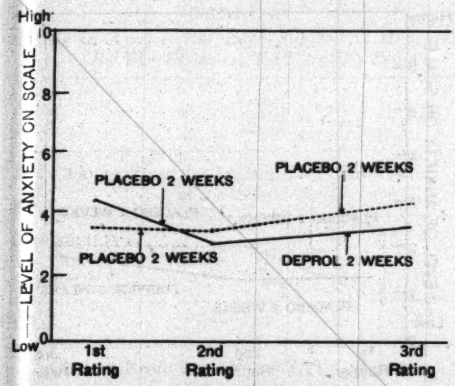

FIG. 1. Change in Level of Anxiety as measured by The Elmhurst Clinical Rating Scale. (Median anxiety level given for each group; ratings are on a ten-point scale.) Control group N = 13; Experimental group N = 13.

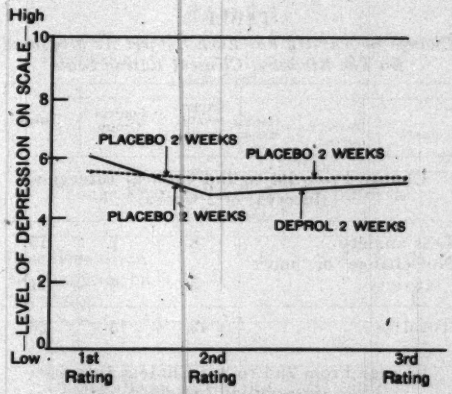

FIG. 2. Change in Level of Depression as measured by The Elmhurst Clinical Rating Scale. (Median depression level given for each group; ratings are on a ten-point scale.) Control group N = 13; Experimental group N = 13.

pleted the study. The data collected consisted of clinical and TAT ratings. Both collections of data were analyzed with respect to anxiety and depression.

Clinical ratings (Figure 1)*:* In analyzing the clinical ratings[6] with respect to change in anxiety level we find that the experimental group, seemed to have a slightly higher level of anxiety at the outset than the control group, and that during the first two-week period, when both groups were on placebo, the experimental group tended to show a drop in anxiety level while the control group remained the same. None of the differences, however, was significant (*t*-test).

Since subjects were randomly assigned, it is assumed that the difference between the two groups at the outset was a function of chance, as were their respective shifts on the second rating. Comparison of the change from the second to the third rating, when Deprol was used in the experimental group, shows no significant difference. Deprol, then, caused no change in anxiety at the clinical level.

The clinical ratings of change in level of depression (Figure 2) show that the groups were at the same level at the outset, had an insignificant drop at the second rating and converged at the third rating. Deprol obviously did not cause a significant decrease in depression at the clinical level.

Another method of analysis of the clinical data was employed, to focus on intra-individual rather than median group change. For intra-individual change in anxiety level (Table 3) there is seen to be no significant difference between groups from the first to the second rating.

Table 4 shows no intra-individual change in depression at the clinical level.

TAT Ratings: In analyzing the TAT, using our own TAT rating scale[7] in relation to change in anxiety level (Figure 3), we find no significant decrease in anxiety. That is, the change from second to third rating for the experimental group is not significantly different from the change that occurred in the control group. This is also the case for level of depression (Figure 4).

The intra-individual change between the three ratings of the TAT material with reference to anxiety (Table 5) and with reference to depression (Table 6) also indicates that Deprol had no significant effect on anxiety and depression.

[6] Scales available upon request.

[7] Available upon request.

TABLE 3

Change in Anxiety For Each Subject As Measured By The Elmhurst Clinical Rating Scale

	Experimental Group	Control Group	Total
Change From 1st to 2nd Clinical Interview (interval of 2 weeks)*			
Less anxiety	8	7	15
No change or more anxiety	5	6	11
Total	13	13	26
Change From 2nd to 3rd Clinical Interview (interval of 2 weeks)†			
Less anxiety	7	4	11
No change or more anxiety	6	9	15
Total	13	13	26

* Subjects in both groups received placebo during this interval.

† Subjects in Experimental Group received Deprol and those in Control Group received placebo during this interval.

TABLE 4

Change in Depression For Each Subject As Measured By The Elmhurst Clinical Rating Scale

	Experimental Group	Control Group	Total
Change From 1st to 2nd Clinical Interview (interval of 2 weeks)*			
Less depression	6	4	10
No change or more depression	7	9	16
Total	13	13	26
Change From 2nd to 3rd Clinical Interview (interval of 2 weeks)†			
Less depression	5	5	10
No change or more depression	8	8	16
Total	13	13	26

* Subjects in both groups received placebo during this interval.

† Subjects in Experimental Group received Deprol and those in Control Group received placebo during this interval.

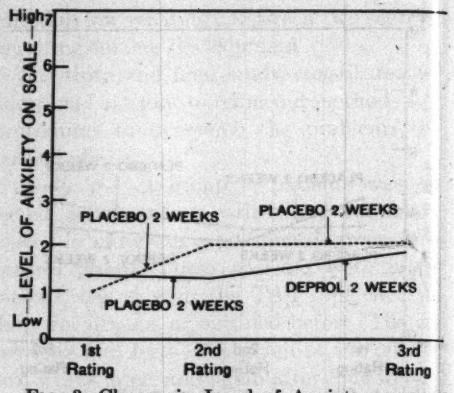

FIG. 3. Change in Level of Anxiety as measured by The Elmhurst TAT Rating Scale. (Median anxiety level given for each group; ratings are on a seven-point scale.) Control group N = 13; Experimental group N = 13.

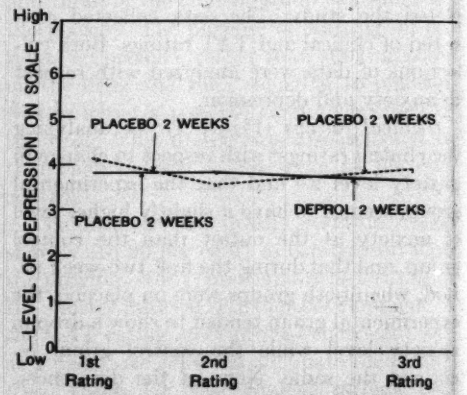

FIG. 4. Change in Level of Depression as measured by The Elmhurst TAT Rating Scale. (Median depression level given for each group; ratings are on a seven-point scale.) Control group N = 13; Experimental group N = 13.

An analysis of intra-individual change between the three ratings of TAT material with reference to depression (Table 7) by yet another method, namely, a thematic content analysis (number of depressive themes per TAT protocol), indicates again that Deprol had no significant effect on depression. These data, analyzed in terms of both median group change and intra-individual change, and from the point of view of clinical behavior as well as at the psychodynamic level, show

TABLE 5

Change in Anxiety For Each Subject As Measured By The Elmhurst TAT Rating Scale

	Experimental Group	Control Group	Total
Change From 1st to 2nd TAT (interval of 2 weeks)*			
Less anxiety	5	2	7
No change or more anxiety	8	11	19
Total	13	13	26
Change From 2nd to 3rd TAT (interval of 2 weeks)†			
Less anxiety	4	5	9
No change or more anxiety	9	8	17
Total	13	13	26

* Subjects in both groups received placebo during this interval.

† Subjects in Experimental Group received Deprol and those in Control Group received placebo during this interval.

TABLE 6

Change in Depression For Each Subject As Measured By The Elmhurst TAT Rating Scale

	Experimental Group	Control Group	Total
Change From 1st to 2nd TAT (interval of 2 weeks)*			
Less depression	5	5	10
No change or more depression	8	8	16
Total	13	13	26
Change From 2nd to 3rd TAT (interval of 2 weeks)†			
Less depression	8	9	17
No change or more depression	5	4	9
Total	13	13	26

* Subjects in both groups received placebo during this interval.

† Subjects in Experimental Group received Deprol and those in Control Group received placebo during this interval.

TABLE 7

Change in Depression For Each Subject as Measured By Thematic Content Analysis of Subjects' TAT Protocols

	Experimental Group	Control Group	Total
Change From 1st to 2nd TAT (interval of 2 weeks)*			
Less depression	8	3	11
No change or more depression	4	9	13
Total	12	12	24
Change From 2nd to 3rd TAT (interval of 2 weeks)†			
Less depression	4	3	7
No change or more depression	8	9	17
Total	12	12	24

* Subjects in both groups received placebo during this interval.

† Subjects in Experimental Group received Deprol and those in Control Group received placebo during this interval.

TABLE 8

TAT Themes of Depression

alone	kill oneself (action or desire)
feeling bad	loneliness
crying	loss
death (including symbols of death such as grave, etc.)	rejection
depression	remorse
destruction	resignation
disappointment	sad (despondent, desolate, gloomy, sorrow, distraught)
grief	separation
guilt	shame
helplessness	unhappiness
hopelessness (despair)	unloved

that Deprol had no significant effect on anxiety or depression.

A further TAT content analysis[8] was conducted, focusing upon manifest content of the theme only, with no effort to interpret

[8] Performed by Henry Lennard, Ph.D., of the Bureau of Applied Social Science, Columbia University, New York

latent content. A list of themes is presented in Table 8.

In this analysis, each answer to the following questions about each TAT card was categorized as to whether or not it contained a depressive theme:

1. What is happening now?
2. How did it begin?
3. How does it end?
4. How do the characters in the story feel?
5. What are they thinking about?

If two depressive themes were referred to in answer to one question, only one (the first) was coded. If two or more themes were given in answer to one question and a distinctly hopeful theme followed the depressive one (*e.g.*, "he felt very sad but now he feels very happy") the depressive theme was not coded.

The results of this manifest content analysis show no significant difference between experimental and control groups.

DISCUSSION AND CONCLUSION

It would appear, from this analysis of the effects of Deprol and placebo by the double-blind method on a group of depressed patients, that Deprol had no measurable effect. With the use of clinical ratings and TAT protocols at three different times, as well as the content analysis of TAT protocols, there appeared to be no significant difference between the effects of the placebo and Deprol. In essence, the experimental findings were consistent with the clinical impression—that Deprol did not seem effective in hospitalized patients or those coming to an out-patient clinic.

The possibility remains that Deprol may be effective on patients in private practice who might be less severely disturbed than the group under study here. Furthermore, our method of study needs to be applied to forms of treatment where there has been stronger empirical evidence of changes, as for instance in patients receiving electroconvulsive treatment. Under such conditions, we expect that our approach will reflect changes consonant with clinical experience. A replication with a larger sample is indicated. In order to obtain dream material for rating and content analysis, more frequent and direct contact must be made with patients by psychologically trained personnel. Future studies, now in a planning stage, will include this procedure.

SUMMARY

A double-blind controlled study of a psychotropic drug, Deprol, was attempted on three levels by clinical ratings, TAT, and by a study of dreams. At the same time an effort was made to study the psychodynamic process of change; and to develop something more than the usual "before and after" measures.

A variety of difficulties were encountered in pilot attempts. Nevertheless, the method employed seems more useful than the conventional approaches to drug evaluation. Deprol appeared not to cause any changes significantly different from a placebo. Larger samples of patients need to be studied by this method with regard to other drugs, and by such established procedures as ECT.

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BRIEF COMMUNICATION

A NOTE ON HYPNOTIC BEHAVIOR, THE DEFINITION OF THE SITUATION AND THE PLACEBO EFFECT

LOUIS B. GLASS, M.A.[1, 2] AND THEODORE X. BARBER, PH.D.[1]

A series of investigations (1–5) has suggested the following hypothesis: a placebo administered as a "hypnosis-producing" drug is as effective as a formal trance induction procedure in enhancing "suggestibility." The following study provided an opportunity to test this hypothesis.

Thirty subjects (20 men and ten women) participated in two experimental sessions (trance induction and control). In the first session (trance induction) the subjects were given the following preliminary instructions by an experimenter (LBG) who had prior training as a hypnotist: "Today you're going to be deeply hypnotized. I believe you'll find it an interesting and enjoyable experience." This was followed by a formal 20-minute trance induction procedure which included as many as practicable of the accepted hypnotic techniques, *e.g.*, eye-fixation, use of a metronome, and repetitive suggestions of relaxation, drowsiness and sleep. Immediately after the trance induction, eight tests of "suggestibility" were administered:

1) *Arm lowering* ("Hold your right arm out . . . the arm is becoming heavy . . . it is moving down").

2) *Arm levitation* ("Hold the left arm out . . . the arm is weightless . . . it is moving up").

3) *Hand lock* ("Clasp your hands together . . . they are welded together . . . it's impossible to get them apart . . . Try, you can't").

4) *Thirst "hallucination"* ("You're very thirsty . . . dry and thirsty").

5) *Verbal inhibition* ("The harder you try to say your name, the harder it will become . . . Try, you can't").

6) *Body immobility* ("Your body is heavy . . . you can't stand up . . . Try, you can't").

7) *Post-hypnotic response* ("When this experiment is over, I'll click like this and you'll blink automatically . . . Every time I click you'll blink").

8) *Selective amnesia* ("You will forget that I told you to be thirsty").

Each test required between 30 and 45 seconds to administer and was scored by objective, pre-established criteria.

Preliminary instructions for the second session (control), held two days to a week later, were: "You will *not* be hypnotized today. I am going to give you some tests of imagination. The better you can imagine and the harder you try, the more you'll respond. Try as hard as you can to concentrate and to imagine the things I tell you." The subject was asked to close his eyes and the same eight ("suggestibility") tests were administered and scored. Following the testing, the subject was requested to compare his experience during the two sessions.

Of the 30 subjects, 20 attained higher scores under the experimental condition (trance induction). (Of the remaining ten, five showed the same score under the two conditions, and five attained higher scores

[1] Medfield State Hospital, Harding, Massachusetts, and Worcester Foundation for Experimental Biology, Shrewsbury, Massachusetts. This investigation was supported by a research grant (MY-3253) from the National Institute of Mental Health of the National Institutes of Health, U.S. Public Health Service. The authors acknowledge with gratitude the assistance of W. Dennis Engels, M.D. in carrying out this study.

[2] Now at the Department of Psychology, University of Kansas, Lawrence, Kansas.

under the control condition.) The question arose, Why did the ("suggestibility") scores of a large proportion (two-thirds) of subjects drop in the second (control) session? The subjects' comments—*e.g.*, "I'm not surprised I did better with hypnosis; after all, the second experiment was only supposed to test imagination"—dovetailed with the findings of earlier experiments (1–5) to suggest that the significant variable affecting the scores was not the presence or absence of the trance induction procedure *per se* but the contrasting manner ("hypnosis" and "tests of imagination") of defining the two sessions. If the definition of the situation was the crucial variable, one could predict that the following would occur in a third session for those subjects whose scores had dropped under the control condition: By defining the situation as "hypnosis"—*without* employing a formal trance induction procedure—scores on the ("suggestibility") tests would again be as high as in the first session (trance induction) and significantly higher than in the second session (control).

To test this prediction, 12 subjects who showed a drop in score under the control condition were asked to participate in a third experiment[3] in which they would be given "a powerful hypnotic drug." All 12 agreed to participate. At the beginning of this session the subject was introduced to a physician who said: "Before I administer the drug I want you to know something about its action. This is a hypnotic drug that acts directly on neurological centers to increase suggestibility without producing sleepiness or relaxation. There is no danger, no side-effects or after-effects. The drug acts quickly but its time of action varies with each individual, and shows up in the diastolic blood pressure, minute pupillary changes, and changes in the reflex pattern. I have the antidote with me, and as soon as the testing is over I will remove all of the effects." Following this, blood pressure measurements were taken, the patellar, supinator and Hoffmann's reflexes were tested and the right eye was examined with an ophthalmoscope, while the physician explained that these were preliminary observations to establish a base line so he could determine when the drug was working. The subject was then given a tablet-and-a-half of an inactive drug (sodium bicarbonate). After the right eye, the blood pressure and the three reflexes had been examined twice, the physician addressed the experimenter: "The subject is ready; go ahead." The experimenter then administered and scored the eight ("suggestibility") tests,[4] with the following results:

1) With one exception, subjects attained higher scores in this session (placebo) than during the preceding session (control). (The mean scores under placebo and control (5.8 and 3.7) differ significantly at the one per cent level of confidence.)

2) Scores were as high in the third session (placebo) as in the first session (trance induction). (Mean scores under the placebo condition and the trance induction condition (5.8 and 6.3) do not differ significantly.)

These results support the following tentative conclusion: Under one order of treatment presentation (trance induction, control, placebo), a placebo administered by a physician as a "hypnosis-producing" drug is as effective as a formal 20-minute trance induction procedure in enhancing "suggestibility." Additional experiments are in progress to determine the validity of the hypothesis under other orders of treatment presentation and under random assignment of treatments to independent groups.

[3] The third session was separated from the second by a two-week interval.

[4] The time elapsing between the administration of the placebo and the administration of the ("suggestibility") tests was three minutes.

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BECK, SAMUEL J. AND MOLISH, HERMAN B. EDS. *Reflexes to Intelligence: A Reader in Clinical Psychology.* The Free Press, Glencoe, Illinois, 1959. xiv + 669 pp., $8.50

Reflexes to Intelligence is a very personal book, revealing much about the tastes, prejudices, hopes, enthusiasms and commitments of its editors. And since its editors are skilled clinicians, dedicated to the enterprise of approaching patients with a real concern for empathic understanding of these people-in-need, the readings in this book convey the spirit of clinical work with patients.

How useful would the book be as a book of readings to supplement some text in a course on clinical psychology? The reviewer cannot imagine that much of this book could be fitted to any current course. Freud's paper on "Instincts and Their Vicissitudes" would fit beautifully into a course on psychoanalysis, motivation or personality. The selection from Binet and Simon's *A Method for Measuring the Intelligence of Young Children* would go with a course in measurement theory, clinical testing or intelligence, but would not fit very well a course on psychoanalysis, motivation or personality. A special course could be devised that would cover all of these fields. A professor considering this book might have to ask whether he wants to develop such a course. And that might not be too bad an idea.

Frank Auld, Jr.

ALEXANDER, G. L. AND NORMAN, R. M. *The Sturge–Weber Syndrome.* Williams & Wilkins, Baltimore, and John Wright and Sons, Bristol, England, 1960. vii + 95 pp., illus. $7.50

One of the most obvious syndromes associated with epilepsy is certainly that described by William Allen Sturge and Parkes Weber. The facial nevus flammeus in these cases is a blazing signpost of the condition. On the basis of seven personal cases and those reported in the world literature, Alexander and Norman discuss the clinical features, the radiographic characteristics, the electrographic concomitants, and the neuropathologic findings of the Sturge-Weber syndrome.

The authors define the syndrome rigidly as characterized by cutaneous facial angiomatosis, epilepsy, gyriform calcifications and, in some cases, buphthalmos. The angiomatosis affects the face and, in part at least, the forehead and upper eyelid; angiomatosis of the lower part of the face, they state, is rarely, if at all, associated with the other manifestations of the Sturge-Weber syndrome. Consequently, they regard any correlation between site of the facial nevus and the cerebral location of the angioma and calcifications as purely fortuitous. The typical wormlike calcifications do not become radiologically visible until after infancy, and do not occur at sites of intracortical vascular malformation, but rather in the cortex underlying an arachnoidal angiomatosis.

The authors conclude that the lesion of the Sturge-Weber syndrome is almost unique in its epileptogenic propensities, and that, once the seizures begin, mental deterioration is almost certain to follow. Accordingly, to avoid the risk of extensive brain damage, surgical removal of the pathologic cerebral tissue is advised not later than two months after birth in the case of an infant seemingly normal except for the characteristic facial nevus of the Sturge-Weber syndrome. Probably few neurologists would accept this conclusion, and only the more courageous neurosurgeons would be willing to operate upon an otherwise apparently normal infant under these circumstances.

The monograph is well written and beautifully illustrated. A full bibliography of the world literature is appended, but there is no index. The volume, unquestionably the best treatise available on the subject, will be particularly valuable to pediatricians, neurologists and neurosurgeons.

A. Earl Walker

SOLLEY, CHARLES M. AND MURPHY, GARDNER. *Development of the Perceptual World.* Basic Books, New York, 1960. xiv + 353 pp., illus. $6.50

This book is an attempt to answer the classic question, Why do we see things as we do?, by examining the function of learning in perception. The authors see the potential value of the book as a stimulus to research. To this reviewer, it seems highly probable that this goal will be achieved.

It is the authors' contention that we may learn to understand perception better if we see its parameters defined in terms of learning as well as of inherent structural components and physiologic units, and that we may understand learning better if we realize the role that a plastic and modifiable process of perceiving may impose upon basic motor learning.

As the authors develop this position, perception becomes excitingly "alive." As a matter of fact, it is the focus on affect and motivation as organizing principles in the act of perceiving which provides the unifying theme of the book. Consequently, a wide variety of concepts and topics are brought together in a most challenging way. Perception as process is analyzed in terms of five stages or molar units: 1) preparatory set or search; 2) the state of the organism just before stimulation, especially with respect to the selectiveness of attention through affective conditioning; 3) the built-in determinants of perception involving the special characteristics of receptor organs, their projection areas, and the interplay between them; 4) a trial-and-check phase involving the period between reception and final percept, with ANS accompaniments; 5) consolidation of the stimulus trace.

Each molar unit is modifiable under the impact of motivation through reinforcement, the perceptual act itself being thereby modified. Motivation is said to regulate set and attention, affect level of sensibility and rate of check, trigger percepts, and have the ability to prevent percepts from being fully structured, as in escape learning. Reinforcement is not used in the sense of drive reducer, but as a stimulus which preserves an act and alters its probability of occurrence. Reinforcers elicit stimuli and emotional-affective responses which stop the perceptual act, reinforcing the preceding act. Percepts which are clear and stable also reinforce the perceptual act which brought them about by stopping the act.

I sketch this outline of some basic tenets in the authors' theory of perception to indicate its character. The book consists of two parts. The first develops the theoretical argument. The second provides an analysis of the molar units of the perceptual act and of how these units are altered through learning. A heterogeneous body of research and observation provides the building blocks for the theoretical fabrication. The chapter on motivation, reinforcement and affect in perceptual learning is built on a broad experimental base which lends substantial support to the conclusion that motivation influences perception through internal stimulation and emotional-affective responses which alter perceptual adaptation and direct, intensify or interfere with the perceptual act. The chapter on autism, however, has little experimental base and seems weak by comparison. It attempts an integration of Piaget and Freudian theory of perceptual development, but is more nearly a recounting than an integration, listing some descriptive characteristics of autism. Although maturation is said to be a key factor in the transition from autistic to veridical perception in the child, its role in the process is not discussed,

and the arguments remain highly "clinical" and conjectural.

This is not intended as criticism, especially since the authors admit at the outset that they will guess, as well as document, in order to present a rounded theory or point of view. Rather, I wish to illustrate the wide range of sources tapped by the agile eclecticism of the authors, and to indicate what must inevitably occur in such a work, *viz.*, that some theoretical points have a firmer empirical basis than do others.

One may wonder whether a work which stretches so far to encompass so much may not be drawn too thin in the process. The authors themselves are not without misgivings. I do not have this reservation myself, but believe that research-based efforts to achieve a theoretic unification of the behaving individual will provide their own reinforcement. Readers of this journal will do well to face the challenge in this book themselves.

David Rosenthal

WOLMAN, BENJAMIN B. *Contemporary Theories and Systems in Psychology*. Harper, New York, 1960. x + 613 pp. $7.50

In a future history of psychology, the mid-twentieth century might well be noted for the last attempts in two kinds of endeavors: the creation of all-embracing theories of psychology, and the writing of textbooks which review such theories. The volume under discussion falls, of course, under the latter rubric, and it attempts to provide "a comprehensive picture of the [sic] contemporary psychological theory." Unfortunately, the result is neither comprehensive, if we look at theories as guideposts and pathfinders to empiric research, nor is it contemporary, if we seek to understand the ideas which currently guide the work of psychologists.

The book is divided into four parts. The first deals with theories oriented "toward natural sciences" and, after an introduction to nineteenth century roots and Pavlovian theory, concentrates on behaviorism and the neo-behavioristic learning theories, ending up with a chapter devoted to hormic and holistic theories. The second part handles psychoanalysis and related systems. Part Three discusses Gestalt, personalistic and field theories and their common roots in German philosophy. The last part is the author's personal contribution to the discussion of scientific method and psychologic theories; unfortunately, it is couched in such vague generalities as to make its utility doubtful.

While it is difficult to quarrel with the author of a comprehensive review book about the catholicity of his selections, some rather glaring deficiencies need to be pointed out. The intention to include only "general" psychologic theories is somewhat (and properly) violated in the section on psychoanalytic theories. However, once this step was undertaken, why was there no treatment, or even mention, of H. A. Murray's system? Why was Piaget's recent work on genetic epistemology excluded when even Binet deserves two pages? Why was there no mention of mathematical learning theory, particularly when Razran's generally uninfluential notions have received more space than Guthrie's?

These questions arise within the author's frame of reference. What is more disturbing is that this book is intended to give the student a picture of "contemporary" theory and systems. Speaking rather generally, it can be suggested that psychology is at the present time growing beyond one of the last heritages of its bondage to philosophy: the need for comprehensive systems. Contemporary psychologic theory is more like theory in other sciences; it is piecemeal, and is adapted to delimited phenomena and areas of interest. For example, current psychologic research gets its theory from such men as Stevens in psychophysics; Estes, Mosteller and Bush in mathematical approaches to learning; Gibson and Ames in perception,

to name just a few "theories and systems" not discussed by Wolman. Neo-Hullian learning theory is removed from the postulate sets by several steps taken by Miller, Spence, Logan, Kendler and Amsel, among others. Gestalt theory and its allies have produced Asch, Wallach, Scheerer and others. Information theory and its application to a variety of behavior play an important part in psychology. In social psychology, the budding psychologist will want to steep himself in Hovland, Festinger, Schachter and others. And so forth, and so forth. In other words, *contemporary* psychology is not directed in the main by the theories and systems set forth in Wolman's volume. In the rush of current research most of his figures have become historical, the forerunners of the present unavoidable fragmentation of psychologic theory.

But how well does Wolman perform a quasi-historical function? Adequately, on the whole, but far from commendably. By ignoring the empiric function of theories, and by failing to evaluate how well they have stood the test of empiric research, he falls far short of doing as fine a job on learning theories as Hilgard has done in *Theories of Learning*, or on personality theories as Hall and Lindzey have done in *Theories of Personality*. Historically he slights Wundt, Ebbinghaus and Titchener in favor of Pavlov and his contemporaries. (It would be hard to determine from this volume that a substantial part of contemporary psychology is concerned with human learning.)

The first impression of scholarship conveyed by the organization of the book (and its 43-page bibliography) is marred by significant omissions which extend into a lengthy treatment of psychoanalysis that stops short of Kris, Levy, Hartmann and Spitz; a discussion of psychologic theory that fails to mention Egon Brunswik; an often recitative approach to theories less familiar to the author; and an annoying habit of self-reference (particularly when it is to unavailable and barely tangential material).

In short, there are better—and more constructively critical—reviews of current theories on the market. Better histories have been written. It is unfortunate that the author failed to address himself to truly "contemporary" psychologic theory. That book is still to be written. Or Wolman could have used his obvious acquaintance with the Continental philosophic antecedents of modern psychology by giving us an expanded version of his Part Three, where both his scholarship and his obvious devotion to problems of psychology could have been used to better advantage. For the time being, I shall continue to refer students to the books cited above, or to Woodworth's short summary of contemporary psychology.

George Mandler

REIFF, ROBERT AND SCHEERER, MARTIN. *Memory and Hypnotic Age Regression. Developmental aspects of cognitive function explored through hypnosis.* International Universities Press, New York, 1959. x + 253 pp. $5.00

One innocent little word, memory, is used to stand for a bewildering diversity of human gifts. Encompassed in this huge realm are the immediate reverberations of experience that linger in our ears, the wealth of personal experience that may be recalled in chronologic order, an even vaster wealth of indifferent, impersonal information, the residues of bodily skill and muscular training, the scars left by long-forgotten yet never outgrown traumatic experience, and much more. Numerous theories (competently reviewed by Reiff and Scheerer) strive to order and account for these phenomena, though none is as yet wholly satisfactory.

In this important area of cognition, considerable controversy rages over the contention of some that, whatever transformations it undergoes, experience is recorded with

considerable veridicality and tenacity, and filed away in largely inaccessible storehouses, but may be recovered in vivid and accurate detail under special circumstances. Penfield's experiences in stimulating the exposed surface of the temporal lobe in conscious patients, who report that sections of past experience unroll automatically before them, point to a dramatic though generally inaccessible method of bypassing the locked door of memory. For decades, hypnotic investigators have claimed that they have a key to this door, with the technique of induced and reversible "age regression." The subject is told, after a deep trance has been attained, that as the experimenter counts (or the like) he will start going backward in time, becoming younger, forgetting everything that has happened since the date the *E* mentions. Hypnotists have been very much impressed by the striking affective and intellectual changes that seem to occur under such conditions: *S* talks, acts and feels like the adolescent or child he has been told he is, and may recover long-unavailable memories and relive the emotional storms of the period. The accounts of some early studies make the reader feel that the hypnotist almost believes that he has invented a time-machine and has really turned the clock back for his *S*.

Naturally there have been many highly vocal skeptics who have pointed out the great willingness of hypnotized *S*s to provide the kind of results *E* wants, and who have claimed that the phenomena produced were nothing more than clever acting. Part of the reluctance to accept the validity of age regression in what have so often been poorly controlled demonstrations has stemmed from its radical implications for a theory of memory. Many psychologists, impressed by the experimental ingenuity of Bartlett, have been inclined to follow his theory of remembering as a constructive and not a simply reproductive process. Bartlett said there are no literal traces of experience that can just be revived, giving a carbon copy of the original occurrence; instead, memories are stored in the form of schemata—loosely organized, dynamic configurations of neural elements, which mediate only approximate recovery of a past that must be rebuilt in the essentially creative process of remembering. If the claims of the hypnotists were accepted, it would be logically necessary to assume that the brain has the capacity to store countless billions of images, a virtually complete sound-movie recording of a person's life—an assumption that was called manifestly absurd.

In most instances, those who worked with the regressed hypnotic *S*s themselves strongly believed they were producing something that could not easily be explained away. Unable to bring their critics into the laboratory where they too could have what Reiff and Scheerer repeatedly aver is a convincing experience—especially in the emotional phenomena elicited—hypnotists have turned to objective tests to demonstrate their points. The results, before the publication of this book, were not particularly convincing; the fact that *S*s who were regressed to age six performed at approximately that level on an intelligence test was quickly rebutted by the demonstration that non-hypnotized *S*s could do at least as good a job of imitating a child's level of performance.

Reiff and Scheerer approached this problem with an innovation so simple and right that one wonders why it was overlooked before. What is distinctively characteristic of the various epochs of childhood, they point out, is not so much the child's quantitative deficiency as his qualitatively different way of going about intellectual tasks. Drawing on their own clinical experience and the brilliant observations of Piaget, they reasoned that, if hypnotic age regression is something more than compliant role-playing, it should be possible to show that the regressed *S* reinstates outmoded *ways* of approaching problems, ways which an adult

is not likely to know characterize the thought of a child of the age specified. In this book they tell the straightforward story of the ingenious experiments in which they tried out this approach, with impressive success.

Five adult "good" hypnotic *Ss* were carefully regressed to ages ten, seven and four, much care and sophistication going into the technique so as to maximize the likelihood of successful regression. Each time, *S* was first taken into a room filled with toys, where he had 10 minutes of free play; then he was tested by a battery of tasks ranging from word association to Piaget's "hollow tube" test.[1] The mode of solution was carefully recorded and, if necessary, inquired into; *Ss* were also questioned about "contemporary" events, such as the names of their teachers and classmates in the second or fifth grade. The results were compared with the performances of three other groups of five adult *Ss* each, all non-hypnotized simulators. Each of the latter was asked to play the role of a 10-year-old child (for example) as well as he could, for the duration of the tests, giving such answers as a child would give.

The treatment of results is exemplary for the meaningful categories used to analyze the mode by which *Ss* achieved or failed the tasks presented, which for the most part was successfully predicted. For example, at "age seven," all *Ss* in both experimental and control groups correctly solved the arithmetic problem, How much is four plus two minus two? But when asked *how* they did it, all of the simulators verbalized the abstract principles of addition and subtraction, one even giving the sophisticated method of simply canceling the last two operations, whereas none of the hypnotically regressed *Ss* used more than a concrete rehearsal of steps: "Take four and add two, gives six, and then take away two, gives four." Most of the eight tasks used yielded one or more statistically significant differences of a predicted kind between experimental and control *Ss*.

It seems a pity that, having done such a brilliant job in the conception of this new approach, the authors did not invest the time and effort that would have been necessary to nail down their points. The most glaring deficiency in the experiment is the lack of any data on real children of the ages studied. Since some of the tasks were original, and few had been given in exactly the form used to children of exactly the ages in question, the authors usually fall back on a more or less plausible argument about the likely performance of children, from their theory of development. How much more convincing it would have been to have corralled a few school-children—only 15 would have done—and put them through just these paces!

The other respect in which this study is disappointing is the use of the control *Ss*. Unfortunately, the experiment was planned before Orne's recent paper was published (*Journal of Abnormal and Social Psychology*, **58**: 277–299, 1959), with its ingenious use of simulating control *Ss*. Orne's technique was superior to Reiff and Scheerer's in two principal respects: the persons who administered the tests and observed the behavior did not know which *Ss* were actually hypnotized and which not; in this respect, it followed the principle of double-blind design. Second, the *Ss* were highly motivated to try to deceive the *E*, having been recruited and instructed by a collaborator; they were also screened for non-hypnotizability. Thus, it was possible for the hypnotist to proceed in exactly the same manner with all *Ss*, only half of whom actually allowed themselves to be hypnotized, the other half role-playing throughout. (Un-

[1] A red, a yellow and a green bead on a string are drawn into a tube and the child is asked to predict which color will emerge first, second and third when they are drawn out of the other end, when they are retracted from the end they entered, and when they are pulled out after the tube has been turned through 180° or 360°. As children mature, Piaget has shown that they are able to perform successively more difficult aspects of this task.

fortunately, Orne did not study age regression in his experiment.) In the present study, apparently both the *E* and the observer who took notes of each *S*'s actions knew the actual status of each *S*, so that all observations were contaminated. No matter how striking the actual differences in affect and behavior, therefore, we have only the unsupported word of the *E* for them, and he is hardly a disinterested party.

The report is slightly marred by a number of minor matters: an occasional overeagerness to interpret tiny differences or to ignore failures of prediction, and a tendency to repetitiveness in writing, which stand out in such a short book. Yet not even the more serious shortcomings pointed out above diminish the essential value of the book: it has provided the most satisfactory evidence to date of the "validity" of hypnotic age-regression, and has indicated something about the nature of the phenomenon. By *validity* the authors are careful to make it explicit they do not mean that they have made time run backward; only that they have reinstituted the "ego stratum prevalent at the pertinent age period." By such a stratum, they mean the configuration of potential remembrances (*i.e.*, personal memories) and also of *memoria*—"memories without the experience of an autobiographic index"—a useful new conceptual distinction, characterizing a particular stage of development, along with the cognitive apparatuses used at that time. The important implication of their work is that each such ego stratum is preserved more or less intact, even though the person has little if any access to it later on.

The reader of this journal for April, 1959 (Klein, G. S. On subliminal activations. JNMD, **128**: 293–301), will catch an echo of other recent experiments: those on the registration of subliminal and marginal experiences (briefly and partially alluded to in this book). The work of such experimenters as Klein and Fisher leads by a different route to the same conclusion: in the new model of the psychic apparatus that we must build to do justice to what is now known about cognitive functioning, provision must be made for the likelihood that a very high proportion of all information put into the human senses, whether accompanied by clear awareness or not, affects the system, is stored, and under the right circumstances can be recovered. We are only beginning to realize how subtle and ramified are the phenomena of memory, and how marvelously intricate must be the neurologic organization that makes possible such impressive phenomena as are reported in this memorable little book.

Robert R. Holt

SAUL, LEON J. *Emotional Maturity: The Development and Dynamics of Personality*, 2nd ed. Lippincott, Philadelphia and Montreal, 1960. xxi + 393 pp. $6.50

This book is warmly recommended as a first reader in psychiatry for medical students, for psychiatrists-in-training who are just beginning their residency work, for persons engaged in the allied behavioral sciences, and for laymen. In a style which is vivid, interesting, and almost completely free from psychiatric jargon, the author presents the ABC's of dynamic psychiatry. He himself clearly possesses the deep understanding of human psychology, normal and abnormal, which is the earmark of the experienced psychoanalyst, and, in addition, he speaks with first-hand familiarity of military psychiatry, penology, physiology and animal psychology. He recurrently spells out the sociologic implications of the knowledge psychiatry has acquired concerning individual psychology.

He gives us an abundance of clinical illustrations, many of them of absorbing interest, and these, whether taken from his psychoanalytic work or from his experience as a military psychiatrist or elsewhere, are presented always with a therapeutic orien-

tation, an orientation that this reviewer found to emerge in refreshing contrast to the comparatively static, largely diagnostic and categorizing frame of reference in which most of the traditional textbooks of psychiatry are written. Throughout his book, the essentially human substance of the illustrative patients comes through in his case descriptions; these are clearly human beings with whom the reader empathizes, rather than fascinating pathologic specimens. There are, for example, few if any passages in this book which will leave the layman with any increased concern about his own neurotic difficulties; rather, he will carry away with him a more open-minded and a healthier attitude toward emotional difficulties, whether in himself or in others.

I found little to quarrel with among the views the author presents concerning his major topics: emotional maturation, emotional forces in the development of personality, the nature of neurosis, and the psychodynamics of personality. His presentation is not only strengthened by references from diverse areas of the professional literature, but also enlivened by a generous number of entertaining and well chosen quotations from the general literature.

On the negative side, it must be emphasized that this is not a book for the psychiatrist who is beyond the residency level of experience; it is decidedly too elementary for him. At times the author's chatty, highly readable style deteriorates into garrulity and the overuse of clichés. There are many overlong paragraphs, at least one covering a page and a half. Finally, nine tenths of the book is devoted not to emotional maturity, as the title would suggest, but to psychopathology.

But these criticisms are minor indeed compared to the high readability and comprehensiveness of the book and, above all, to the author's clearly compassionate and therapeutic orientation toward as well as deeply perceptive understanding of troubled human beings in their unceasing struggle toward the always relative emotional maturity.

In such passages as this the author seems, naively, to believe that preventive psychiatry offers a panacea for all the complex ills of adult life:

> "... The crime, the wars, the abysmal selfishness and the cruelty which form the scene in human history are essentially *childish* qualities, the reactions of children to mistreatment during their early years. These should be outgrown normally and could be if children were reared properly from birth" (p. 19).

The author's optimism, however, well tempered in the main, forms one of the strongest aspects of his book. The young psychiatrist in particular will find a special inspiration here, the inspiration to share with Dr. Saul the keen pleasure of doing psychoanalytic work, a pleasure seldom explicitly acknowledged in the professional literature:

> "... The true core of the personality is usually remote from the person's consciousness. The main trends usually are better seen by discerning outsiders than by the person himself. Only the dynamically trained expert with a grasp of unconscious motivations and processes can dissect, observe, describe and appreciate these constellations in their flow, detail and dynamic balance. To do so is the greatest thrill in psychiatry...." (p. 181).

Harold F. Searles

INDEX, VOLUME 132

A

Abnormal EEG discharge, verbal and conditioned avoidance learning during, 50
Abnormal states of muscle tone, 508
Abnormalities, serum protein, in psychosis, 89
Abramson, Harold A., 358
Accidents caused by alcoholic drivers, personality versus intoxication as critical factor in, 298
Acquisitive, sexual and aggressive deviates, 44
Admissions, psychiatric, psychological characteristics of and implications for treatment, 324
Adult, object loss in, 388
Adult rearrangement and neonatal deprivation, 28
Affleck, D. Craig, 335
Aggressive, sexual and acquisitive deviates, 44
Agnosia, sensory deprivation, sensory suppression and: Neurologic theory, 32
Alcoholic drivers, personality versus intoxication as critical factor in accidents caused by, 298
Alcoholic personality, the, 299
Alcoholism: The Nutritional Approach, 453
Alexander, G. L., 542
Alienation, social, and personal disorganization, inter-individual differences in degree of, 101
Amphetamine withdrawal psychosis, clinical and biochemical studies, 234
Analysis of déjà vu, a contribution to, 91
Androgen deficiency: Castration, 242
Androgen: The libido hormone, 244
Anniversary hypothesis, the, 3, 11
Anxiety hierarchies, construction of, 192
Anxiety, stress and, 385
Aptitude, role-changing, and recognition of human images in hysterics, 315
Arrested development (faulty learning type), symbiotic psychosis as, 400
Art and Illusion: A Study in the Psychology of Pictorial Representation, 344
Arteriosclerosis, chronic cerebral, effects of isoxsuprine hydrochloride (Vasodilan) on, 335
Assessing social contact on a psychiatric ward, a method for, 277
Autobiographical content in confabulation, 365
Avoidance learning, conditioned, and verbal learning during abnormal EEG discharge, 50

B

Barber, Theodore X., 539
Basal ganglia in Parkinsonism, visual motor performance before and after chemosurgery of, 307
Basic Theory of Psychoanalysis, 359
Beaghler, H. Edward, 522
Beck, Samuel J., 542
Beecher, Henry K., 175, 448
Bellak, Leopold, 531
Belmont, Ira, 410
Bennett, Lillian F., 457
Bertino, Joseph, 404
Biochemical and clinical studies of an amphetamine withdrawal psychosis, 234
Biochemistry: serum protein abnormalities in psychosis, 89
Biology of Myelin, The, 181
Birch, Herbert G., 410
Bodenheimer, A. R., 181
Brain: chronic cerebral arteriosclerosis, effects of isoxsuprine hydrochloride (Vasodilan) on, 335
Brain-damaged (hemiplegic) patients, functional levels of disturbance manifested by, as revealed in Rorschach responses, 410
Brengelmann, J. C., 98
Brown, Fred, 176
Brown, Harold M., Jr., 522
Bruch, Hilde, 114
Buchwald, Jennifer S., 146

C

Callaway, Enoch, III, 404
Campbell, Robert J., 447
Capacity of levels of nervous system to function independently, 75
Case of multiple personality illustrating the transition from role-playing, a, 497
Castration and hypogonadism (dry-run orgasm), 289
Castration: Androgen deficiency, 242
Cerebral arteriosclerosis, chronic, effects of isoxsuprine hydrochloride (Vasodilan) on, 335
Changes in patterns of intelligence test performance associated with homonymous visual field defects, 227
Changes, perceptual, in sensory deprivation: Suggestions for a conative theory, 17
Chapman, Jean P., 185
Chapman, Loren J., 171
Chappell, Dorothy T., 432
Chemosurgery of basal ganglia in Parkinsonism, visual motor performance before and after, 307
Child's Conception of Geometry, The, 94

Chlorpromazine (Thorazine) in psychiatric treatment, 425
Chromosomal sex, hormones and, 247
Chronic cerebral arteriosclerosis, effects of isoxsuprine hydrochloride (Vasodilan) on, 335
Circuit, cultural, in psychology and psychiatry, 127
Clergy, hospitalized, factors in mental illness among, 80
Clinical and biochemical studies of an amphetamine withdrawal psychosis, 234
Clitorectomy in hermaphrodites, 293
CNS: gamma efferent system, conditioned responses in, 146
Colby, Kenneth Mark, 451
Comparative studies of the phenothiazine tranquilizers: Methodological and logistical considerations, 61
Comparative study of selected tranquilizers in treatment of psychiatric patients, 425
Comparison of fluphenazine, trifluoperazine and a placebo in the context of an active treatment unit, 522
Compazine (prochlorperazine) in psychiatric treatment, 425
Components of eroticism in man:
I. Hormones in relation to sexual morphology and sexual desire, 239
II. Orgasm and genital somesthesia, 289
Conative theory, suggestions for, of perceptual changes in sensory deprivation, 17
Conceptual deficit in schizophrenia, nature of, 118
Conceptual problems in psychiatry. Psychiatry and philosophy of science. I:, 485
Conceptual problems in schizophrenia, 114
Conceptual problems, recognition of in psychiatry, 486
Conceptual thinking of schizophrenics, critical review of research, 118
Conceptualization, importance of in the spiral of science, 485
Conceptualization, the two major errors of: Inflexibility and unreliability, 487
Conditioned avoidance and verbal learning during abnormal EEG discharge, 50
Conditioned responses in the gamma efferent system, 146
Confabulation content sources, 364
Confabulation in the Wernicke-Korsakoff syndrome, 361
Congdon, Mark H., 497
Congenital indifference to pain, 249
Construction of anxiety hierarchies, 192
Contact, social, on a psychiatric ward, a method for assessing, 277
Contemporary Theories and Systems in Psychology, 544
Contribution to the analysis of déjà vu, a, 91
Convulsive therapy, sociopsychological characteristics of patients who refuse, 153
Coordination and perception, exposure-history as a factor in maintaining stability of, 2
Correlates of life history in schizophrenia, 22
Cranial X irradiation, maze performance and fixated behavior in rat after, 30
Creativity and the Individual, 452
Critical factor in accidents caused by alcoholic drivers, personality versus intoxication as, 298
Critical review of research on conceptual thinking of schizophrenics, a, 118
Critique, McKeever's, a reply to (validity of the Hewson ratios), 420
Critique of Wolff's study (validity of the Hewson ratios), 417
Crothers, Bronson, 446
Cultural circuit in psychology and psychiatry, 127
neurophysiology and psychology of, 127
psychopathology, 140

D

Danehy, John J., 182
David, Henry P., 98
Death, sudden, phenomenon of, 386
Defect experiments, 36
Deficit, conceptual, in schizophrenia, nature of, 118
Definition of the situation, placebo effect and hypnotic behavior, 539
Deformities (sexual), social handicap of, 240
Déjà vu, a contribution to analysis of, 91
Deliberate falsification, 362
Delivery room difficulties, some social psychological aspects of, 515
Depression and psychosomatic disease, 389
Depression, study of the effects of Deprol on: a method of a psychodynamic process study of psychotropic drugs, 531
Depressive psychoses, schizophrenia and psychoneuroses, evidence for functional genesis in, 3
Deprivation
and rearrangement, 26
maternal, in the neonate, 387
neonatal, adult rearrangement and, 2
sleep, 387
Deprivation, sensory
facts in search of a theory, 17
perceptual changes in: Suggestions for a conative theory, 17
sensory suppression and agnosia: Neurologic theory, 32
studying perceptual development using the technique of, 21

Deprol, effects of on depression: A method of a psychodynamic process study of psychotropic drugs, 531
Deranged temporal frame, 370
Desensitization treatment, systematic, of neuroses, 189
Desire
hormonal mechanism and, 241
male-female differences of, hormones and, 246
sexual, and sexual morphology, hormones in relation to, 239
starvation and, 241
Deutsch, Felix, 348
Deutsch, J. A., 448
Development of the Perceptual World, 543
Development, perceptual, studied using the technique of sensory deprivation, 21
Deviants, sexual, incarcerated, group therapy of, 158
Deviates, sexual, aggressive and acquisitive, 44
Differences, inter-individual, in degree of personal disorganization and social alienation, 101
Differences of desire, male-female, hormones and, 246
Dilantin (diphenylhydantoin), leukopenia with, 339
Diller, Leonard, 307
Diphenylhydantoin, leukopenia with, 339
Disarrangement and noise (visual), 26
Discharge, abnormal EEG, verbal and conditioned avoidance learning during, 50
Discussion (sensory deprivation symposium), 40
Disease and the Advancement of Basic Science, 448
Disorganization, personal, and social alienation, inter-individual differences in degree of, 101
Disturbance manifested by brain-damaged (hemiplegic) patients, functional levels of, as revealed in Rorschach responses, 410
Doehring, Donald G., 227
Drinking and Intoxication, 98
Drivers, alcoholic, personality versus intoxication as critical factor in accidents caused by, 298
Drop-outs in drug study, 69
Dynamic Psychopathology in Childhood, 350
D'Zmura, Thomas L., 101

E

Editorial, 1
EEG discharge, abnormal, verbal and conditioned avoidance learning during, 50
Effects of isoxsuprine hydrochloride (Vasodilan) on chronic cerebral arteriosclerosis, 335
Efferent system, gamma, conditioned responses in, 146
Ejaculation and emission differentiated, 289
Eldred, Earl, 146
Eliasberg, Wladimir G., 182
Emission and ejaculation differentiated, 289
Emotional Maturity: The Development and Dynamics of Personality, 2nd ed., 548
Encyclopedia of Witchcraft and Demonology, The, 175
Epilepsia, 269
Equanil (meprobamate) in psychiatric treatment, 425
Erikson, Erik H., Selected Papers, 353
Erlebnisgestaltung, 181
Eroticism in man, components of
I. Hormones and sexual morphology and sexual desire, 239
II. Orgasm and genital somesthesia, 289
Error of unreliability, the, 489
Errors, reification and other, of inflexibility, 487
Errors, the two major, of conceptualization: Inflexibility and unreliability, 487
Evidence for functional genesis in mental illness: Schizophrenia, depressive psychoses and psychoneuroses, 3
Existentialism, phenomenology and, in psychiatry: An historical analysis, 469
Exposure-history as a factor in maintaining stability of perception and coordination, 26

F

Factors in mental illness among hospitalized clergy, 80
Factors, personality and life history, in persons who are potentially hypertensive, 457
Faulty learning type of arrested development, symbiotic psychosis as, 400
Feather, Ben W., 485
Feibleman, James K., 127
Feifel, Herman, 177
Female hermaphrodites, hyperadrenocortical, orgasm in, 291
Female-male differences of desire, hormones and, 246
Fessel, W. J., 89
Field defects, visual, homonymous, changes in patterns of intelligence test performance associated with, 227
Fink, Max, 153
Finn, Michael H. P., 176
First Five Minutes, The: A Sample of Microscopic Interview Analysis, 182
Fixated behavior and maze performance in the rat after cranial X irradiation, 304
Fluphenazine, trifluoperazine and a placebo,

comparison of in the context of an active treatment unit, 522
Folie, the hungan and, 263
Folk psychiatry in Haiti, 260
Freedman, Lawrence Z., 44
Freedman, Sanford J., 17
Frohman, Charles E., 234
Frost, Thomas T., 304
Functional genesis in mental illness, evidence for: Schizophrenia, depressive psychoses and psychoneuroses, 3
Functional levels of disturbance manifested by brain-damaged (hemiplegic) patients as revealed in Rorschach responses, 410
Further studies on speech patterns of schizophrenic patients: Personal disorganization and social alienation, 101

G

Gamma efferent system, conditioned responses in, 146
Garner, Ann M., 185
Genesis, functional, in mental illness, evidence for: Schizophrenia, depressive psychoses and psychoneuroses, 3
Genital functions, reflex, in paraplegics, 293
Genital somesthesia, orgasm and (Components of eroticism in man, II), 289
Genital somesthesia: Tactual stimulation, 292
Giedt, F. Harold, 324
Glass, Louis B., 539
Gleser, Goldine C., 101
Gombrich, E. H., 344
Gordon, Benjamin Lee, 181
Gottschalk, Louis A., 101
Group Processes, 273
Group therapy of incarcerated sexual deviants, 158
Gulliksen, H., 266

H

Hain, Jack, 497
Haiti, folk psychiatry in, 260
Hall, Bernard H., 184
Hallucination and imagination, 78
Handbook on Mental Illness for the Catholic Layman, A, 270
Handicap, social of (sexual) deformities, 240
Hanlon, Thomas E., 61
Harris, Harry, 347
Harris, Robert E., 457
Hartmann, Heinz, 446
Hebb, Donald O., 40
Heinze, Shirley J., 452
Held, Richard, 26
Hemiplegic brain-damaged patients, functional levels of disturbance manifested by, as revealed in Rorschach responses, 410
Hermaphrodites, clitorectomy in, 293
Hermaphrodites, female hyperadrenocortical, orgasm in, 291
Heroic Sanctity and Insanity: An Introduction to the Spiritual Life and Mental Hygiene, 97
Herrick, Howard D., 335
Hewson ratios, validity of
critique of Wolff's study, 417
reply to McKeever's critique, 420
Hierarchies, anxiety, construction of, 192
Hierarchies, examples of from cases, 196
Hilgard, Josephine R., 3
Hine, Frederick R., 485
Hinsie, Leland E., 447
Historical analysis of phenomenology and existentialism in psychiatry, an, 469
History, life, correlates in schizophrenia, 221
Hockett, Charles F., 182
Homonymous visual field defects, changes in patterns of intelligence test performance associated with, 227
Hormonal mechanism and desire, 241
Hormone for libido, the (androgen), 244
Hormones
and male-female differences of desire, 246
and sex-chromosome status, 247
and sexual morphology and sexual desire, (Components of eroticism in man, I), 239
sex, and sex desire, 241
Hospitalized clergy, factors in mental illness among, 80
Howell, Robert J., 522
Human Biochemical Genetics, 347
Human images, recognition of, and role changing aptitude of in hysterics, 315
Humans, influence of varying dosage on effects of lysergic acid diethylamide in, 404
Hungan, the, and folie, 263
Hyperadrenocortical female hermaphrodites, orgasm in, 291
Hypertensive, personality and life history factors in persons who are potentially, 457
Hypnosis
induction of, suggestibility with and without, 204
use of in traumatic paraplegia, 432
Hypnotic behavior, A note on, definition of the situation and the placebo effect, 539
Hypogonadism, castration and, 289
Hysteria, Reflex and Instinct, 453
Hysterics, role changing aptitude and recognition of human images in, 315

I

Identity and the Life Cycle (Selected Papers of Erik H. Erikson), 353

Images, human, recognition of, and role changing aptitude in hysterics, 315
Imagination; hallucination, 78
Imaginative elaboration, 363
Implications for treatment, psychological characteristics of psychiatric admissions and, 324
Importance of conceptualization in the spiral of science, 485
Impotence, orgasm despite after priapism, 292
Incarcerated sexual deviants, group therapy of, 158
Independence of lower centers, 77
Independent function of levels of the nervous system, 75
Indifference to pain, congenital, 249
Induction of hypnosis, suggestibility with and without, 204
Infant, object loss in, 388
Inflexibility and unreliability: Two major errors of conceptualization, 487
Inflexibility, reification and related errors of, 487
Influence of varying dosage on effects of lysergic acid diethylamide (LSD-25) in humans, 404
Inhelder, Bärbel, 94
Intelligence test performance, changes in associated with homonymous visual field defects, 227
Inter-individual differences in degree of personal disorganization and social alienation, 101
Intoxication, personality versus, as critical factor in accidents caused by alcoholic drivers, 298
Introduction to Psychoanalytic Research, An, 451
Irradiation, X, cranial, maze performance and fixated behavior in the rat after, 304
Isolation studies, 33
Isoxsuprine hydrochloride (Vasodilan), effects of on chronic cerebral arteriosclerosis 335

J

Jaco, E. Gartley, 183
Jessner, L., 350

K

Kahn, Jennie B., 185
Kalis, Betty L., 457
Kantor, Robert E., 221
Kellam, Sheppard G., 277
Kelley, Harold H., 346
Kiev, Ari, 260
Klee, Gerald D., 404
Kløve, Hallgrim, 227
Kollar, Edward J., 382
Korey, Saul R., 181
Korsakoff's and Wernicke's disease, confabulation in syndrome, 361
Kretschmer, Ernst, 453
Kubie, Lawrence S., 2
Kurland, Albert A., 61
Kurtzke, John F., 339

L

Learning Theory and Behavior, 454
Learning type, faulty, of arrested development, symbiotic psychosis as, 400
Learning, verbal and conditioned avoidance, during abnormal EEG discharge, 50
Lennox, William, 269
Leukopenia with diphenylhydantoin, 339
Levels of the nervous system and their capacity to function independently of each other, 75
Levin, Max, 75
Libido hormone, the (androgen), 244
Life history and personality factors in persons who are potentially hypertensive, 457
Life history correlates in schizophrenia, 221
Loss of parent and anniversary hypothesis, 10
Lothrop, William W., 118
Lower centers, independence of, 77
Lunacy, a search for, 171
Lysergic acid diethylamide, influence of varying dosage on effects of, in humans, 404

M

Macmillan, M. B., 397
Magee, Kenneth R., 249
Magliocco, E. Bruno, 101
Male-female differences of desire, hormones and, 246
Marconi, Juan, 315
Masor, Nathan, 344
Masserman, Jules H., 449
Maternal deprivation in the neonate, 387
Matrix, description of, 278
Mautner, Hans, 355
Maze performance and fixated behavior in the rat after cranial X irradiation, 304
McAllister, Robert J., 80
McCarthy, Raymond G., 98
McKeever, Walter F., 417
McKeever's critique, reply to (validity of the Hewson ratios), 420
Meaning of Death, The, 177
Measurement of Subjective Responses: Quantitative Effects of Drugs, 175
Medieval and Renaissance Medicine, 181
Memory and Hypnotic Age Regression, 545
Mendelson, Myer, 267
Menninger, Karl, Selected Papers, 184

Mental illness among hospitalized clergy, factors in, 80
Mental illness, evidence for functional genesis in: Schizophrenia, depressive psychoses and psychoneuroses, 3
Mental Retardation: Its Care, Treatment and Physiological Base, 355
Meprobamate (Deprol), 531
Meprobamate (Equanil), 425
Messick, S., 266
Method for assessing social contact, a: Application during a rehabilitation program on a psychiatric ward, 277
Milstein, Victor, 50
Minority, A: A Report on the Life of the Male Homosexual in Great Britain, 269
Molish, Herman B., 542
Money, John, 239, 289
Moore, Thomas Verner, 96
Morphology and social maturity, 239
Morphology, sexual, and sexual desire, hormones in relation to, 239
Mother-Child Interaction in Psychosomatic Disorders, The, 185
Motor performance, visual, before and after chemosurgery of basal ganglia in Parkinsonism, 307
Mowrer, O. Hobart, 454
Multidrug studies, 61
Multiple personality, a case of, illustrating the transition from role-playing, 497
Muñoz, Leonardo, 315
Murphree, Oddist D., 304
Murphy, Gardner, 543
Muscle tone, abnormal states of, 508
Muscle tone, spasticity, rigidity, 505

N

Nacht, S., 266
Native theories of psychiatric illness (Haiti), 261
Natural History of Cerebral Palsy, The, 446
Nature of conceptual deficit in schizophrenia, 118
Neonatal deprivation, adult rearrangement and, 28
Neonate, maternal deprivation in, 387
Nervous system, levels of and their capacity to function independently, 75
Neurologic theory notes on sensory deprivation, sensory suppression and agnosia, 32
Neurophysiology and psychology of the cultural circuit, 127
Neurophysiology: muscle tone, spasticity, rigidity, 505
Neuroses, systematic desensitization treatment of, 189
New Psychiatry, The, 344
Newman, Martha F., 3
Noise (visual) and disarrangement, 26
Nonhuman Environment, The, 356
Norman, R. M., 542
Note on hypnotic behavior, the definition of the situation and the placebo effect, a, 539

O

Object loss in infant and in adult, 388
On the Mysterious Leap from the Mind to the Body, 348
Orgasm
despite impotence after priapism, 292
in hyperadrenocortical female hermaphrodites, 291
Orgasm and coordinate variables: Emission and ejaculation differentiated, 289
Orgasm and genital somesthesia (Components of eroticism in man, II), 289
Orgasm, dry-run:
Castration and hypogonadism, 289
Prostatectomy, 290
Oyarzún, Fernando, 315

P

Pain, congenital indifference to, 249
Paine, Richmond, 446
Palombo, Stanley, 114
Panic, the problem of, 392
Paraplegia, traumatic, a psychological approach to: Use of hypnosis, 432
Paraplegics, reflex genital functions in, 293
Parent loss and anniversary hypothesis, 10
Parkinsonism, visual motor performance before and after chemosurgery of basal ganglia in, 307
Patient movement—Drop-outs, 69
Patients, Physicians, and Illness, 183
Patients who refuse convulsive therapy, sociopsychological characteristics of, 153
Patterns of intelligence test performance, changes in associated with homonymous visual field defects, 227
Pavenstedt, E., 350
Pavlovian approach to symbiotic psychosis, 397
Pavlovian theory and schizophrenia, 399
Penectomy, 294
Perception and coordination, exposure-history as a factor in maintaining, 26
Perceptual changes in sensory deprivation: Suggestions for a conative theory, 17
Perceptual development studied using the technique of sensory deprivation, 21
Permitil (fluphenazine), a comparison of with trifluoperazine (Stelazine) and a placebo in an active treatment unit, 522
Perphenazine (Trilafon) in psychiatric treatment, 425
Personal disorganization and social alienation, inter-individual differences in degree of, 101

Personality, alcoholic, 299
Personality and life history factors in persons who are potentially hypertensive, 457
Personality, multiple, a case illustrating the transition from role-playing, 497
Personality versus intoxication as critical factor in accidents caused by alcoholic drivers, 298
Perspectives in Personality Research, 98
Phenomenology and existentialism in psychiatry: An historical analysis, 469
Phenothiazine tranquilizers, comparative studies of: Methodological and logistical considerations, 61
Phenothiazine tranquilizers: comparison of fluphenazine, trifluoperazine and a placebo in an active treatment unit, 522
Philosophy of science, psychiatry and. I: Conceptual problems in psychiatry, 485
Piaget, Jean, 94
Pittenger, Robert E., 182
Placebo, comparison of fluphenazine, trifluoperazine and, in an active treatment unit, 522
Placebo effect, the definition of the situation, and hypnotic behavior, 539
Pollack, Max, 153
Pool, Chalmers S., 304
Potentially hypertensive, personality and life history factors in persons who are, 457
Practice of Clinical Child Psychology, The, 271
Priapism, orgasm despite impotence after, 292
Problem of panic, 392
Problems in psychiatry, conceptual. Psychiatry and philosophy of science. I., 485
Process study of the effects of Deprol on depression: a method of a psychodynamic process study of psychotropic drugs, 531
Prochlorperazine (Compazine) in psychiatric treatment, 425
Prostatectomy (dry-run orgasm), 290
Protein abnormalities in serum of psychotics, 89
Psychanalyse, La (Vol. 5), 268
Psychiatric admissions, psychological characteristics of, and implications for treatment, 324
Psychiatric Dictionary, 3rd ed., 447
Psychiatric patients, comparative study of selected tranquilizers in treatment of, 425
Psychiatric ward, social contact on, a method for assessing, 277
Psychiatrist's World, A, 184
Psychiatry and philosophy of science. I: Conceptual problems in psychiatry, 485
Psychiatry
and psychology, the cultural circuit in, 127
folk, in Haiti, 260
phenomenology and existentialism in: An historical analysis, 469
recognition of conceptual problems in, 486
Psychoanalysis and Moral Values, 446
Psychoanalysis of Today, 266
Psychoanalytic Concepts of Depression, 267
Psychodynamic process study of psychotropic drugs: A process study of the effects of Deprol on depression, 531
Psychodynamic themes of incarcerated sexual deviants, 166
Psychological approach to traumatic paraplegia: Use of hypnosis, 432
Psychological aspects of delivery room difficulties, social, 515
Psychological characteristics of psychiatric admissions and implications for treatment, 324
Psychological Scaling: Theory and Applications, 266
Psychological stress: A re-evaluation, 382
Psychology and neurophysiology of the cultural circuit, 127
Psychology and psychiatry, the cultural circuit in, 127
Psychoneuroses, schizophrenia and depressive psychoses, evidence for functional genesis in, 3
Psychopathology of the cultural circuit, 140
Psychopharmacology
amphetamine withdrawal psychosis, clinical and biochemical studies of, 234
chlorpromazine (Thorazine), 425
Compazine (prochlorperazine), 425
Deprol (meprobamate), 531
Equanil (meprobamate), 425
fluphenazine (Permitil), 522
isoxsuprine hydrochloride (Vasodilan) effects on chronic cerebral arteriosclerosis, 335
LSD-25, effects of varying dosage in humans, 404
meprobamate (Deprol), 531
meprobamate (Equanil), 425
Permitil (fluphenazine), 522
perphenazine (Trilafon), 425
prochlorperazine (Compazine), 425
Stelazine (trifluoperazine), 522
Thorazine (chlorpromazine), 425
tranquilizers, phenothiazine, comparative studies of, 61
trifluoperazine (Stelazine), 522
Trilafon (perphenazine), 425
Psychophysiology: leukopenia with diphenylhydantoin, 339
Psychosis, amphetamine withdrawal, clinical and biochemical studies, 234

Psychosis, serum protein abnormalities in, 89
Psychosis, symbiotic
a Pavlovian approach to, 397
as arrested development (faulty learning type), 400
as regressive disorder, 401
Psychosomatic disease and depression, 389
Psychotherapy and Society, 182
Psychotropic drugs, a method of a psychodynamic process study of (effects of Deprol on depression), 531

R

Radiation, *see irradiation*
Rat, maze performance and fixated behavior in after cranial X irradiation, 304
Rearrangement, adult, and neonatal deprivation, 28
Rearrangement, deprivation and, 26
Recognition of conceptual problems in psychiatry, 486
Recognition of human images, role changing aptitude and in hysterics, 315
Recombination experiments, 34
Reflex genital functions in paraplegics, 293
Reflexes to Intelligence: A Reader in Clinical Psychology, 542
Regressive disorder, symbiotic psychosis as, 401
Reification and related errors of inflexibility, 487
Reiff, Robert, 545
Reitan, Ralph M., 227
Relaxation, training in, 1
Reply to McKeever's critique (validity of the Hewson ratios), 10
Research on conceptual thinking of schizophrenics, critical review of, 492
Reticent patients, 118
Riese, Walther, 109
Riesen, Austin H., 469
Rigidity, muscle tone, spasticity, 21
Riklan, Manuel, 505
Robbins, R. H., 307
Role changing aptitude and recognition of human images in hysterics: A preliminary study, 175
Role-playing, a case of multiple personality illustrating the transition from, 315
Roman, Ruth E., 497
Rorschach responses, functional levels of disturbance manifested by brain-damaged (hemiplegic) patients as revealed in, 266
Rosengren, William R., 410
Rosenhan, David, 515
Rosenzweig, Norman, 531
Ross, Alan O., 249
271

S

Salk, Lee, 531
Saul, Leon J., 548
Schaffner, Bertram, 273
Scheerer, Martin, 545
Schizophrenia
conceptual problems in, 114
depressive psychoses, psychoneuroses, and, evidence for functional genesis in mental illness, 3
nature of conceptual deficit in, 118
Pavlovian theory and, 399
Schizophrenia: Correlates of life history, 221
Schizophrenic patients, speech patterns of: Personal disorganization and social alienation, 101
Schizophrenics, conceptual thinking of, critical review of research, 118
Schneck, Jerome M., 91
Schneider, Stanley F., 249
Science and Psychoanalysis, Vol. II: Individual and Familial Dynamics, 449
Science, psychiatry and philosophy of. I: Conceptual problems in psychiatry, 485
Science, spiral of, importance of conceptualization in, 485
Search for lunacy, a, 171
Searles, Harold F., 356
Selzer, Melvin L., 298
Sensory deprivation: Facts in search of a theory, 17
Sensory deprivation
discussion, 40
perceptual changes in: Suggestions for a conative theory, 17
sensory suppression and agnosia: Notes for a neurologic theory, 32
studying perceptual development using the technique of, 21
Sensory suppression, sensory deprivation and agnosia: Neurologic theory, 32
Serum protein abnormalities in psychosis: A preliminary report, 89
Sex hormones and sex desire, 241
Sex-chromosome status, hormones and, 247
Sexual, aggressive and acquisitive deviates, 44
Sexual deviants, incarcerated, group therapy of, 158
Sexual morphology and sexual desire, hormones in relation to (Components of eroticism in man, I), 239
Shea, Daniel, 270
Simopoulos, Aris M., 61
Simson, Clyde B., 234
Sjoberg, Bernard M., Jr., 204
Sleep deprivation, 387
Social alienation and personal disorganiza-

tion, inter-individual differences in degree of, 101
Social contact on a psychiatric ward, a method for assessing, 277
Social handicap of deformities (sexual), 240
Social psychological aspects of delivery room difficulties, 515
Social Psychology of Groups, The, 346
Société Française de Psychanalyse, La, 268
Sociopsychological characteristics of patients who refuse convulsive therapy, 153
Sokolow, Maurice, 457
Solari, Guido, 315
Solley, Charles M., 543
Some social psychological aspects of delivery room difficulties, 515
Somesthesia, genital, orgasm and (Components of eroticism in man, II), 289
Spasticity, muscle tone, rigidity, 505
Speech patterns of schizophrenic patients: Personal disorganization and social alienation, 101
Spiral of science, importance of conceptualization in, 485
Spring Grove comparative phenothiazine study, 63
Stability of perception and coordination, exposure-history as a factor in maintaining, 26
Starvation, 386
Starvation and desire, 241
Stein, Morris I., 452
Stelazine (trifluoperazine), a comparison of with fluphenazine (Permitil) and a placebo in an active treatment unit, 522
Stevens, Janice R., 50
Stevenson, Ian, 497
Stress and anxiety, 385
Stress, definition of, 390
Stress models, 382
Stress, psychological, re-evaluation, 382
Structural Basis of Behavior, The, 448
Studying perceptual development using the technique of sensory deprivation, 21
Sturge-Weber Syndrome, The, 542
Substitution therapy lapsed:
Men, 243
Women, 244
Sudden death, phenomenon of, 386
Suggestibility, 379
Suggestibility with and without induction of hypnosis, 204
Suppression, sensory, sensory deprivation and agnosia: Neurologic theory, 32
Symbiotic psychosis
a Pavlovian approach to, 397
as arrested development (faulty learning type), 400
as regressive disorder, 401
Systematic desensitization treatment of neuroses, the, 189
Szeminska, Alina, 94

T

Talland, George A., 361
Task content and structure for schizophrenics, 119
Tatom, Mary H., 61
Temporal frame, deranged, 370
Teuber, Hans-Lukas, 32
Thibaut, John W., 346
Thinking, conceptual, of schizophrenics, critical review of research, 118
Thomas, Juergen E., 505
Thorazine (chlorpromazine) in psychiatric treatment, 425
Toms, Esther C., 425
Training for Clinical Psychology, 176
Training in relaxation, 191
Tranquilizers, phenothiazine, comparative studies of: methodological and logistical considerations, 161
Tranquilizers, selected, comparative study of in treatment of psychiatric patients, 425
Traumatic paraplegia, a psychological approach to: Use of hypnosis, 432
Treatment, implications for, psychological characteristics of psychiatric admissions and, 324
Treptow, Keith R., 335
Trifluoperazine, fluphenazine and a placebo, comparison of in an active treatment unit, 522
Trilafon (perphenazine) in psychiatric treatment, 425
Two major errors of conceptualization: Inflexibility and unreliability, 487

U

Unconscious Before Freud, The, 352
Unreliability and inflexibility: Two major errors of conceptualization, 487
Unreliability, the error of, 489
Use of LSD in Psychotherapy, The, 358

V

Validity of Hewson ratios
Critique of Wolff's study, 417
Reply to McKeever's critique, 420
Vanderveldt, Albert, 80
Vasodilan (isoxsuprine hydrochloride), effects of on chronic cerebral arteriosclerosis, 335
Verbal and conditioned avoidance learning during abnormal EEG discharge, 50
Verisimilitude, coherence, stability, 365

Visual field defects, homonymous, changes in patterns of intelligence test performance associated with, 227
Visual motor performance before and after chemosurgery of basal ganglia in Parkinsonism, 307
Vulvectomy, 295

W

Waelder, Robert, 359
WAIS, validity of Hewson ratios
critique of Wolff's study, 417
reply to McKeever's critique, 420
Weintraub, Walter, 404
Weitzenhoffer, André M., 204
Wenar, Charles, 185
Wernicki–Korsakoff syndrome, confabulation in, 361
Westwood, Gordon, 269
Whyte, Lancelot Law, 3[illegible]2
Williams, Roger J., 4[illegible]3
Winder, Clarence L., 2[illegible]1
Withdrawal psychosis, amphetamine, clinical and biochemical studies, 2[illegible]4
Wolff, B. Berthold, 4[illegible]0
Wolff's study, critique of (validity of the Hewson ratios), 4[illegible]7
Wolman, Benjamin B., 5[illegible]4
Wolpe, Joseph, 1[illegible]9

X

X irradiation, cranial, maze performance and fixated behavior in the rat after, 304

Y

Yalom, Irvin D., 158
Young, Gregory G., 234

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Table of Contents

PERSONALITY AND LIFE HISTORY FACTORS IN PERSONS WHO ARE POTENTIALLY HYPERTENSIVE. *Betty L. Kalis, Robert E. Harris, Lillian F. Bennett and Maurice Sokolow* 457

PHENOMENOLOGY AND EXISTENTIALISM IN PSYCHIATRY: AN HISTORICAL ANALYSIS. *Walther Riese* 469

PSYCHIATRY AND PHILOSOPHY OF SCIENCE: I. CONCEPTUAL PROBLEMS IN PSYCHIATRY. *Frederick R. Hine and Ben W. Feather* 485

A CASE OF MULTIPLE PERSONALITY ILLUSTRATING THE TRANSITION FROM ROLE-PLAYING. *Mark H. Congdon, Jack Hain and Ian Stevenson* ... 497

MUSCLE TONE, SPASTICITY, RIGIDITY. *Juergen E. Thomas* 505

SOME SOCIAL PSYCHOLOGICAL ASPECTS OF DELIVERY ROOM DIFFICULTIES. *William R. Rosengren* 515

A COMPARISON OF FLUPHENAZINE, TRIFLUOPERAZINE AND A PLACEBO IN THE CONTEXT OF AN ACTIVE TREATMENT UNIT. *Robert J. Howell, Harold M. Brown and H. Edward Beaghler* 522

A PROCESS STUDY OF THE EFFECTS OF DEPROL ON DEPRESSION. *Leopold Bellak, Lee Salk and David Rosenhan* 531

BRIEF COMMUNICATION. *Louis B. Glass and Theodore X. Barber* 539

BOOK REVIEWS 542

INDEX 551

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